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Journal
of
The Franklin Institute

Devoted to Science and the Mechanic Arts

Vol. 199 JANUARY, 1925 No. 1

THE STEAM TURBINE—AS A STUDY IN APPLIED PHYSICS.*

BY

HON. SIR CHARLES A. PARSONS, K.C.B., M.A., LL.D., D.Sc., F.R.S.

Franklin Medalist, Member of the Institute.

THE subject of this address makes it perhaps a matter of interest to recall a few instances from past history bearing upon the association of physics with the advancements in the arts and sciences.

James Watt, whose great work was in engineering, was by such association led to discover more about the properties of steam, which were imperfectly known at the time; he had many friends among the physicists and chemists, but their knowledge of the subject, in which he was deeply interested, did not satisfy him. He, therefore, experimented himself and drew his own conclusions, which, though only approximate, were yet sufficiently correct to guide him to his epoch-making achievements. Then again, there can be no doubt that the work of Watt influenced physicists to direct their attention to the accurate determination of the properties and laws of steam—and not long afterwards to the establishment of the great laws of thermodynamics; for Joule's great work was in physics, although he began with an endeavor to improve an electromagnetic engine worked by the electrolytic consumption of zinc, which he at first thought would supersede the steam engine of Watt as a prime mover; in this work he showed the highest skill as an electrical and mechanical

* Address delivered Thursday, September 18, 1924, on the occasion of the centenary of the founding of The Franklin Institute.

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engineer. Although he failed in his attempt, his experiments had the result of leading him to the great discovery of the mechanical equivalent of heat, and the absolute zero of temperature. It would seem probable that had he not at first attempted an impossible physical and electrical engineering problem, he might never have determined the mechanical equivalent of heat.

Again, Sadi Carnot was drawn towards his great work on thermodynamics by watching the Watt steam engines, some of which were then working in France, and by pondering on the function of the steam as the working fluid, how it acted within the cylinder, what was the heat cycle, and what was the relation between the heat of the fuel burnt under the boiler and the work done. He pondered over these things and eventually evolved the solution, the "Carnot cycle." He was not, however, fully aware of the mechanical equivalent of heat or of the absolute zero of temperature; his solution was in consequence only approximate, but the accurate solution followed later. The reconciliation of the discoveries of Joule and Carnot was subsequently made by Clausius, Rankine and others. Thus were the great fundamental laws of thermodynamics discovered.

When Sir Isaac Newton was asked how he made his discoveries, he replied, "By always thinking unto them, I keep the subject constantly before me, and wait till the first dawnings open slowly by little and little, into a full and clear light." And he adds, "If I have done the public any service this way, it is due to nothing but industry and patient thought." Sir Oliver Lodge here remarks: "This is the way—quiet, steady, continuous thinking, uninterrupted and unharassed brooding. Much may be done under those conditions—all the best-thinking work of the world has been thus done."

Faraday, in his lecture on the forms of matter to the City Philosophical Society of London in 1819, when he was twenty-seven years of age, said:

> "The disagreeable and uneasy sensation produced by incertitude will always induce a man to sacrifice a slight degree of probability to the pleasure and ease of resting on a decided opinion, and where the evidence of a thing is not quite perfect, the deficiency will be easily supplied by desire and imagination. The efforts a man makes to obtain a knowledge of nature's secrets merit, he thinks, their object for their reward; and though he may, and in many cases must, fail of obtaining his desires, he seldom thinks himself unsuccessful, but substitutes the whisperings of his own family for the revelations of the goddess."

When Newton, at the age of twenty-three, discovered that the motions of the solar system were due to the action of a central force, varying inversely with the square of the distance, his theory was based on Kepler's laws and was clear and certain, and he might have rushed into premature publication after our nineteenth century fashion. But that was not his method. He tried whether the force of gravity would serve and he found that it was 15 per cent. too strong, owing to incorrect numerical data as known at that time, and he thereupon laid aside all thought of the matter for sixteen years, when new and more accurate determinations of gravity were available which enabled theory to be reconciled with observation.

The methods followed by Newton were daring, but restrained by caution; as indeed were those of Faraday, but Faraday's warning seems to indicate that other methods of thought and procedure were prevalent in his day, which indeed seems to have become still more common in our time, in science, in physics and chemistry, and more especially in engineering and in the arts. So much is this the case, that the name of inventor has become distasteful to those who have done really useful work and the best work, in engineering and in the arts, or as Newton has said—"have done the public any service this way."

There is undoubtedly a strong tendency induced by constantly thinking about one subject—to view it with distorted perspective and incorrect magnification, thus leading to wrong and unjustifiable conclusions, as so well described by Faraday. In this connection, it is interesting to consider the mental attitude and method of the skilled and really useful inventor. The inventor must have a wide and intimate acquaintance with the properties of all the materials with which he has to work and by long and patient thought must endeavor to raise in his mind a picture of the ideal combination he is seeking to form out of the materials at his disposal. In the search he will picture in his mind edifice after edifice, combination after combination, machine after machine, and examine how far they conform to the known laws of architecture, physics, chemistry and engineering; some may be seen at once to contravene those laws and are discarded, while others more plausible will go to the touchstone of mathematical calculation and tentative experiment, and, should this reveal no imperfection or palpable defect, then to more rigorous or costly research

and experiment. In short, the skilled inventor marshals his ideas and, following the procedure of nature, puts them through a process of "Natural Selection" with the object of obtaining the fittest solution of his problem. The testing out of any discovery or invention is probably, in general, the most difficult and the most important part of the work, a work in which a knowledge of physics is of inestimable value; in most cases, it is a matter of hard work and patience. That some scientists and many inventors are prone to jump to conclusions too readily, seems to be principally due to the lack of scientific knowledge and methods of thought among inventors. Invention is, in almost all cases, the result of the work of many persons and very rarely that of one man.

Sir Berkley Monihan, when reviewing the great advances that have been made in surgery in recent times, summed up as follows: "A discovery is rarely the work of one mind. It is one observation added to another that makes the supersaturated solution from which the crystal of truth at last precipitates."

The development of the steam turbine, to which I have directed much attention, may perhaps be taken as an illustration of a research in one class of engineering, and probably as representative of researches in many other lines of manufacture. At the outset, the collection of new data was obviously required before the general line of advance could be determined. Some preliminary experiments were made with high-speed shafts and bearings, but in order to complete these data a small turbine coupled to a high-speed dynamo of primitive design was made: The calculated stresses due to centrifugal force, the laws governing the flow of steam and data from dynamos as approximately known at that time, were taken into account. This machine was tested out on very similar lines to those followed by Joule when trying to improve the electromagnetic engine of Sturgeon. The constants for the flow of steam, the loss by friction in bearings at high surface speeds, the hysteresis and eddy current losses in armature core, conductors, and binding wire at abnormally high speeds, were approximately investigated.

Higher mathematics were not employed in this initial work, but were used much later to coördinate the accumulated data and forecast the effect of improvements and refinements which have, in recent years, considerably increased the thermal efficiency of the

turbine; as a matter of fact, it does not now appear that the use of higher mathematics in the earlier stages of development would have been useful; the accumulation of sufficiently accurate data to enable them to be practically and usefully applied would have been at that time an additional burden and a hindrance to progress. This, however, does not imply that a mathematical and physical training is not of very great value, for the two men who directed the work had passed mathematical and physical courses at universities, as well as being trained engineers. All that it is intended to emphasize is that mathematics should be used in the proper place, and in engineering and pioneer work, chiefly to consolidate the rear, assist the communications, but seldom to lead in the advance.

On the other hand, the coming of the steam turbine has had the effect of stimulating mathematical and physical research in certain directions, notably in the dynamics of rotating shafts, the law of flow of saturated and superheated steam through nozzles, and the frictional resistance to flow through passages and over surfaces, also to supercooling and other phenomena.

Present-day knowledge of both the theory and the technique of steam engines has justified Watt in the principles which he laid down. The main development of the steam engine since his time has consisted in an extension of the range of expansion and in an ever-increasing degree of compounding. The separation of the condenser from the cylinder by Watt was followed by the division of the expansion into stages in separate cylinders, finally into four stages in the highest development of the reciprocating engine. The steam turbine carries this subdivision still further, the number of stages included in a reaction turbine of high efficiency being commonly sixty or more. But whilst in the compound reciprocating engine the primary object of compounding is to reduce the temperature range of each individual cylinder, and so to diminish the condensation and loss which occurs when hot steam is admitted into a cylinder, the walls of which have been cooled to the temperature of the steam just discharged from it, in a steam turbine there is no such cyclic variation of temperature. The extensive compounding which is now adopted in all steam turbines of high efficiency is nevertheless of vital importance to their performance.

The necessity for it arises primarily from the low density of

the working fluid, in consequence of which, if the steam velocity is to be kept proportionate to the linear velocity of the blades, only a small fraction of the available energy per pound of fluid can be dealt with in each stage. Since the available energy of the steam is first of all converted into kinetic energy, it follows that this must be done in a large number of stages. The ratio of blade velocity to steam velocity is thus easily controlled by the adoption of a suitable degree of compounding. In large turbines of the reaction type, the velocity of the blades is nearly equal to that of the steam. Under such conditions, the efficiency of reaction blading is practically equal to the efficiency of discharge of well-shaped nozzles.

The high efficiency of which the steam turbine is capable, the widely extended pressure and temperature range which it can utilize without mechanical difficulties, and the fact that it can be built for very large power output from one unit, have made it the ideal prime mover for electric generating stations. Fortunately, the relation between the output and speed of an alternator follows nearly the same law (the inverse square) as the steam turbine and the combination of turbine and alternator thus rapidly progressed in size to meet the demands for large units and in all large power stations using fuel, steam turbines now provide the motive power, with generating units ranging in capacity up to 60,000 kw. Some of these stations now have a total capacity of supply of 500,000 kw. feeding into widespread distribution systems and covering large areas.

In marine work, there has been a similarly rapid progress both in economy and capacity, the marine turbine having advanced in output during twenty years' development from the 2000 S.H.P. for the *Turbinia* to the 150,000 H.P. of H.M.S. *Hood*.

The first marine turbines were directly coupled to the propeller shafting, but all marine turbines built in recent years are connected to their propeller through some form of gearing. The steam turbine being essentially a high-speed engine, the association with a low-speed propeller put the marine turbine at a disadvantage so long as it remained directly coupled to the propeller shaft, so that its early development was restricted largely to the propulsion of high-speed vessels such as warships, liners and channel steamers; although in such cases the direct coupled turbine soon established its superiority over the reciprocating engine.

With the introduction of mechanical gearing in 1910, the marine turbine was freed from this limitation. The turbine has now been applied for the propulsion of all classes of sea-going vessels and considerable improvement in economy effected by the increased efficiencies of both turbines and propeller when these are allowed each to rotate at its most efficient speed. The general result is that steam consumptions have been reduced to less than one-half of that of the early direct coupled turbines.

Whilst mechanical gearing was introduced in the first place with the object of making the turbine applicable to low-speed vessels, it was found to be of value even in higher speed vessels, and the direct coupled turbine may now be said to have been completely superseded by the geared turbine in all classes of later construction.

Mechanical gearing has also assisted in the extension of the field of application of turbines on land. Continuous current generators which have been made in sizes up to 3000 kw. at moderate speeds can now be driven by efficient high-speed turbines, as also can low-frequency alternators and alternators of small or moderate output. The general turbine has also been used to a considerable extent for the driving of mills, such as textile, paper and jute mills. In these installations the high economy of the turbine and the powerful starting torque to be obtained with it are of great value, and so also is the uniform turning moment of a turbine drive of particular advantage in mills where the power is transmitted to the different floors by ropes.

Now, let us consider the theoretical position of the steam turbine. At the present time, the materials at our disposal do not allow in practice a higher temperature than about 750° F. for the superheater, and it has become necessary to search for means of increasing the efficiency of the thermodynamic cycle without increasing the maximum steam temperature.

There are at least four different ways of doing this. These are as follows:

(1) Diminishing the amount of heat thrown away in the condensing water, by utilizing steam withdrawn from the turbine for heating the boiler feed water.

(2) Increasing the mean temperature of heat reception by increasing the boiler pressure.

(3) Increasing the energy and the stage efficiency of the low-pressure part of the expansion by reheating the steam after partial expansion.

(4) Arranging the exhaust and portions of the blading so as to expand the steam efficiently to the highest vacuum obtainable in the condenser.

Considering these proposals in the order given above, it will be evident, on theoretical grounds, that the necessity to heat the feed water, in other words, to reheat the water of condensation before conversion at constant temperature into steam, is the defect of the Rankine cycle; because it involves heat reception at temperature below the maximum.

Since the steam in the course of its expansion through the turbine is all the time falling in temperature, it is clear that this defect could be overcome if the heating of the feed could be accomplished by transfer of heat from the steam at corresponding stages of equal temperature, or, in other words, by a regenerative process. With the addition of such a process, the cycle would be thermodynamically reversible, and under such conditions, the efficiency of the Rankine cycle for saturated steam would be brought up to that of the Carnot cycle. In practice a close approximation to this regenerative process can be obtained by the employment of a sufficient number of feed-water heaters in cascade supplied with steam tapped off from suitable stages of the turbine.[1] The steam which thus transfers its heat to the feed heater is first of all made to do some work by expansion in the turbine down to the temperature at which it is required for withdrawal to the corresponding heaters, and since a certain amount of heat is required for the feed water in any case, the work obtained from this tapped-off steam is obtained merely at the expense of additional heat equal to the work done by it, in other words, this additional heat is utilized at nearly 100 per cent. efficiency. Expressed in another way, the utilization of some of the heat of the steam to preheat the feed water reduces the amount of heat that has to be taken in by the boiler and also that rejected to the condenser.

A large number of such stages of feed heating introduces

[1] Feed heating in a single stage by partly expanded steam is a well-known expedient. It was proposed by James Weit in 1876 and by Normand in 1889; and feed heating in progressive stages was proposed by Ferranti in 1906.

some complications in the pipe work involved, but even with two or three stages very considerable heat economy can be effected. Generally speaking, economizers are used for the final heating of the feed water, but there is a tendency to develop the cascade system further so as to carry the steam heating of the feed water to the highest possible temperature. The economizer being thus replaced, the residual heat of the flue gases of the boiler plant can be most advantageously utilized for regenerative preheating of the incoming air for the furnace.

Another method of improving the efficiency of the thermodynamic cycle is to increase the mean temperature of heat reception by increasing the boiler pressure.

The heat that is absorbed during the process of evaporation, *viz.*, the latent heat of the steam, is a very large proportion of the total heat transmitted to it by the boiler and superheater, and in order that this transmission may take place at a high temperature, the boiler pressure must be high. With a boiler pressure of 250 pounds per square inch, by gauge, the temperature of evaporation is 406° F., and assuming a final temperature of 750° F. after superheating and that by the use of feed heaters in cascade, the water is fed into the boiler at the boiling point, the mean temperature of heat reception from the furnace is about 430° F. If, however, the boiler pressure is increased to say 2000 pounds per square inch, the temperature of evaporation is 637° F., and with the same superheat temperature as before, owing to the fact that the latent heat is now received at a higher evaporation temperature, the mean temperature is considerably increased, *viz.*, to 680° F.

When adopting a high pressure such as that just mentioned, it appears advisable to have a small auxiliary high-pressure turbine embodying several stages of small diameter running at high revolutions and exhausting into the turbines at what is now ordinary boiler pressure. Such high-pressure turbines are actually being built, one in Great Britain for 1500 pounds per square inch and one in the United States for 1250 pounds per square inch.

A third method of increasing the efficiency of the thermodynamic cycle is to reheat the steam after partial expansion in the high-pressure stages of the turbine. It is clear that an improvement in efficiency of the cycle might be made, if after

superheat to the maximum temperature, further heat were added so as to maintain isothermal expansion at this maximum temperature throughout the initial stages of the turbine, carrying it to such a point that subsequent adiabatic expansion in the turbine to the condenser pressure would leave the steam just saturated at that pressure. This has long been recognized as the theoretical ideal in that direction, but as a practical approximation to it, there is being adopted in some installations at the present time a reheating of the steam which, after a certain amount of expansion in the turbine, is extracted from the turbine and raised again to a high temperature in a reheater and led back to the turbine to continue its expansion. Such additional heat is added at a temperature higher than would otherwise have been the mean temperature of heat reception of the working fluid.

Reheating has the additional and important advantage of keeping the steam longer in a superheated condition, and therefore diminishing the range where moisture and consequent loss by water resistance occur.

While it may be said that the temperature of the water available for cooling imposes a limit to the utilization of high vacuum, yet the latter is an important factor in the design of a turbine, since it involves the necessity of providing ample blade area and passageway for the great volume of low-pressure steam. The volumetric expansion of the steam in its passage through a turbine is provided for partly by increase in the area of the blade passages and partly by increase in velocity. With the high peripheral speeds adopted in most low-pressure blading at the present time, while the steam speed may be such as to give a perfectly satisfactory velocity ratio, yet it may entail a loss of kinetic energy of considerable amount in the steam leaving the terminal blades.

A simple solution for the exhaust area problem in large machines is to provide a separate low-speed turbine of large diameter for the final stages of the expansion arranged in close proximity to the condenser.

Application of all the foregoing principles is embodied in a turbine plant of 50,000 kw. capacity which has been built at Newcastle-on-Tyne for the new Crawford Avenue Power Station, Chicago. This plant has been described and illustrated

in the contemporary press to which I may refer those desirous of studying the technical details. It is perhaps interesting to add that a thermal efficiency from fuel to electricity is anticipated about equal to that of the best internal combustion engine.

An appendix is added giving the estimated consumption of

Table Showing the Overall Thermal Efficiencies Which It Is Estimated Could Be Realized with Increased Boiler Pressures up to 2000 Pounds per Square Inch.
(Based on the Callendar Tables and Formulæ for the Properties of Steam.)

Column No.	1	2	3	4	5	6	7
Case No.	S.V.P. Lbs. Gauge.	S.V.T. °F.	°F. Initial Superheat.	Assumed Reheat Pressure Lbs. Abs.	Reheat Temp. °F.	Restored Superheat °F.	Exhaust Vacuum Ins. Hg. Bar. 30.0".
I	250	750°	344°	65	700°	402°	29.25"
II	500	750°	381°	100	700°	372°	29.25"
III	1,000	750°	204°	150	700°	343°	29.25"
IV	1,500	750°	153°	250	700°	299°	29.25"
V	2,000	750°	114°	400	700°	254.5°	29.25"

Column No.	8	9	10	11	12	13
Case No.	Stage Feed Water Heating from 65° F. up to (°F.).	Thermal Efficiency (from Steam to Electricity).	Assumed Boiler Plant Efficiency (Including All Aux.).	Overall Thermal Efficiency (from Fuel to Electricity).	Per Cent. Reduction in Fuel Consumption.	Equivalent Lbs. Oil B.H.P. Hr.
I	360°	31.6	84.0	26.52	....	0.501
II	420°	33.5	83.5	27.97	5.45	0.475
III	510°	35.2	83.0	29.20	10.08	0.455
IV	550°	36.6	82.5	30.20	13.82	0.440
V	600°	37.5	82.0	30.75	15.90	0.432

S.V.P. — Stop Valve Pressure.
S.V.T. — Stop Valve Temperature.
Feed water heated to within about 50° F. of boiler temperature by means of steam withdrawn from turbine.
Blading at several points.
Boilers equipped with economizers or air preheaters or both, so as to recover heat from the flue gases and maintain high thermal efficiency.
Dynamo efficiency taken as 96.5 per cent. at full rated output.
Gross caloric value of fuel oil taken as 18,500 $\frac{\text{BTAU.}}{\text{Lb.}}$

fuel oil per brake horsepower burnt under the boilers at various boiler pressures up to 2000 pounds per square inch.

Applying the same principles to marine work, it is interesting to inquire what results can be obtained using high temperature and boiler pressure, stage feed heating and air preheating but without intermediate reheat.[2]

[2] The adoption of reheat after partial expansion would involve carrying a steam pipe of large capacity twice through the bulkhead separating engine room from boiler room, and this, though not impossible, is open to objection.

In such an arrangement, it is desirable, in order to obtain good efficiency in the high-pressure range, to adopt a separate fast running high-pressure turbine. The three-turbine arrangement, consisting of high-pressure, intermediate- and low-pressure turbines geared to the propeller shaft, is specially applicable under such conditions. Assuming such a design to be adopted for an installation of about 5000 S.H.P. with boiler pressure 500 pounds, superheat temperature 700° F., feed heating to 350° F., and effective air preheating to give a boiler efficiently when fired by fuel oil of about 84 per cent., a careful estimate based on known performance shows that owing to the increased available energy of the steam, a fuel consumption of about .57 pound per S.H.P. would be obtained for turbines only, and .69 pound per S.H.P. including auxiliary machinery.

When the difference in price between fuel oil and Diesel oil is taken into consideration, these being in about the ratio of 3 to 4, and also allowance made for the cost of lubricating oil for the Diesel engine in excess of that for the turbine, it will be seen that these figures are equivalent to about .38 pound of Diesel oil per S.H.P. for turbines only, and .475 pound Diesel oil per S.H.P. for all purposes, including auxiliary machinery, and therefore promise an economy in cost of fuel superior to that obtained with Diesel engines; whilst the first cost of such an installation will be little, if any, greater than that of a geared turbine installation of the usual design with low pressures and moderate superheat, and considerably less than the cost of an oil engine.

To sum up: By practical application of the principles discussed above to the expansion of steam in a turbine, overall thermal efficiencies can be realized which are not inferior to those of internal combustion engines.

It will also be seen that by raising the boiler pressure to 2000 pounds per square inch, the Carnot cycle is closely approached and the maximum possible efficiency obtained consistent with the limits of temperature assigned by the endurance of metals at high temperatures.

Atomic Weight of Antimony.—PHILIP F. WEATHERILL, of the University of Michigan (*Jour. Am. Chem. Soc.*, 1924, **46**, 2437–2445), has determined the atomic weight of antimony by the analysis of antimony trichloride. The value obtained is 121.748 ± 0.00086.

J. S. H.

SPECTROSCOPY IN THE PAST AND IN THE FUTURE.*

BY

PROFESSOR CHARLES FABRY.

University of Paris,

Franklin Medalist, Member of the Institute.

SUCH a ceremony, devoted to the celebration of the past and of the future of an important scientific institution, invites us to cast a glance on the past of scientific research and to try to guess, by extrapolation, what it can be in the future. I will try to make such a survey rapidly, of spectroscopy, a relatively old science (older than its name) whose development (like an American city) has been extraordinarily rapid in these last years.

It has been so rapid, in fact, that it is now difficult to trace its limits, because it has joined surrounding boroughs. It can be defined as a study concerning all radiations and their relations to matter, either in experimental laboratory work or in astronomy. Defined in that way, the field of spectroscopy includes about half of physics and the greater part of astronomy, as most of the knowledge which we have of the stars comes from an analysis of their light.

Curiously enough, the experimental means used in this large field of research have not grown nearly as rapidly as the size of the field itself. The best way to follow the development will be to see what are the necessary parts of an experiment in spectroscopy and to report on their past and present state, and the desiderata of the future.

In any experiment in spectroscopy we have three necessary parts:

(1) A source of radiation (natural or artificial);

(2) An apparatus to separate the radiations (dispersing apparatus);

(3) A receiving apparatus to disclose and record the existing radiations.

THE RESOLVING APPARATUS.

The rôle of this apparatus is to resolve, into separate images, the different radiations existing in the incident beam in order to project these separate images into the receiving apparatus.

* Address delivered Thursday, September 18, 1924, on the occasion of the observance of the centenary of the founding of The Franklin Institute.

An ideal resolving device should have many different qualities. Its resolving power should be infinite, that is, it should be able to separate two radiations, however small the difference between their wave-lengths. It should give a direct measurement of the wave-length. Its light-gathering power should be enormous in order to permit the investigation of the weakest light. It should be able to transmit any kind of radiation without absorption. Useless to say, such an ideal apparatus exists no more than any other ideal.

The existing types of apparatus are of four different kinds; each one has some especially good qualities with the lack, however, of some others, and can therefore be used only in a part of the spectroscopic work. *The prism* is the oldest, and possibly still the most extensively used kind of dispersing apparatus. Its resolving power proceeds from the inequality of velocities of the different radiations in a transparent medium except *in vacuo*. The separation, of course, can occur only if the path in the medium is sufficiently long, and that very simple remark leads to the famous theorem of Lord Rayleigh: The resolving power of a prism is proportional to the thickness of the path of light in the prism (it being understood that the prism is used to the edge). Great thickness of the transparent material is consequently necessary in order to have a great resolving power. Besides, in order to increase the light-gathering power, large surfaces are used. Both those conditions lead to the use of very large masses of transparent dispersive materials, which cause great difficulties, due to the lack of homogeneity and of transparency, the strains in solids and the difference in temperature producing the same effects as a lack of homogeneity.

Since the time of Newton, the construction of prisms has, of course, made great progress, but in the last thirty years it has not made progress in conformity with the general increase in technical knowledge. The material still in most general use is glass (just as it was two and one-half centuries ago); for many years the production of optical glass has not shown much improvement, especially as regards transparency and homogeneity. We must confess that the spectroscopists are very exacting people concerning these two qualities. A resolving power of 100,000 is good but not excessive in many cases; it can be attained and surpassed by the use of gratings. To have it with a dense flint

prism in the blue part of the spectrum, a thickness of twenty inches would be necessary, and I fear that not much blue light would come through twenty inches of dense flint as obtainable at present. Moreover, the lack of homogeneity (natural or by temperature differences) would be so small as to fail to produce an inequality of optical path of a quarter of a wave-length.

On the other hand, glass is not transparent at all for short waves, and we are dependent on crystals (quartz, fluorite) which are not as large nor as homogeneous as one would wish. For waves shorter than 1200 Ångströms no material for prisms is available.

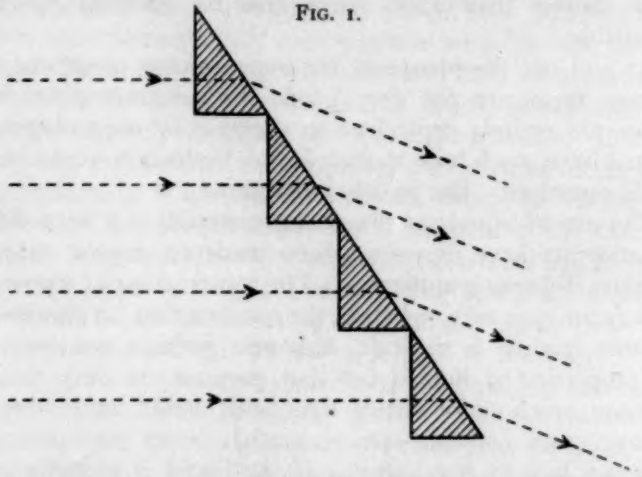
As you see, the prospects for improvement of prisms of the ordinary types are not very good; the spectroscopists, in that respect, are entirely dependent on chemists or mineralogists, and I do not have much hope in their help. Perhaps it would be better to help ourselves. But in what direction?

The use of liquids as dispersing materials is a very old idea; few attempts have, however, been made to employ them as a first-class dispersing apparatus. The construction of a poor liquid prism is an easy task, whereas the construction of prisms of the desirable quality is difficult, although perhaps not impossible. The properties of liquids for that purpose are very attractive. We have much more choice than with solids, more dispersion in many cases (about twice as much), better transparency, no strain, no lack of homogeneity if the liquid is properly stirred. To take the same figure as formerly, a resolving power of 100,000 in the blue, with a CS_2 prism, will be obtained with a thickness of ten inches. The greatest difficulty arises from the difference or change in temperature, the effect of which is to greatly change the refracting index. In the last example, a difference in temperature of one-thousandth of 1° C. between the edge and base of the prism would seriously injure the resolving power. Consequently, we must keep the temperature perfectly constant throughout the entire volume of the prism, and also in time, if we use photography. That is not a hopeless problem with the present-day technical means. I must mention that some good results have recently been obtained with the use of water prisms by M. Duclaux.

Another way of improving prisms would be to try prisms consisting of several pieces of glass or other solid material. Some interesting trials have been made; it would be worth while to

seriously consider them. One can easily imagine an arrangement of many small prisms placed in parallel and producing a wide beam with a small thickness of glass (Fig. 1). If one wishes only to increase the gathering power of the light, all the small prisms should only give the same deviation and the same dispersion. This could certainly be obtained without any great expense. The problem is much more difficult if one wishes to increase also the resolving power, because the different waves must not only have the same direction, but also the same phase. When this is achieved

FIG. 1.



for one radiation it will not be maintained for the other radiation and the pattern will have some analogy with the Michelson echelon.

Grating.—Chronologically, the second type of dispersing apparatus is the grating, used first by Fraunhofer, just a century ago.

It was first used as dispersing apparatus because it allows the wave-length to be measured directly (which is not done by a prism); it proved later, especially through the splendid work by Rowland forty years ago, much more efficient than existing prisms with regard to resolving power; and more recently, in the hands of Lyman, it has shown itself able to supersede prisms for short waves when transparent material is no more available. Consequently the importance of gratings in modern spectroscopy is very great.

Unfortunately, the grating, in comparison with prisms, has

some disadvantages, the greatest of which is a much smaller light-gathering power. That explains why the most of the spectroscopic work on stars has been done with prisms.

The construction of gratings is an extremely difficult problem; only very few machines have been successful, and it cannot be said that perfection has been attained. I do not think that any existing grating is entirely free from false spectra, so-called ghosts. The difficulty is very different from that encountered in prisms; it is not a question of material, it is a purely mechanical problem. The spectroscopist is not, in this case, dependent on the chemist, but on the mechanical engineer. We see the way to great improvement open as regards the perfection, the resolving power and the light-gathering power (dependent chiefly on the size) of the grating. Much work, ingenuity and money have already been spent on that problem; the amount of money spent, I think, can be expressed, with a thousand dollars as a unit; if it could be expressed with a million dollars (a familiar unit to your industrial people) probably many more results could be obtained.

Interferential Apparatus.—No theoretical difference can be made between a grating and an interferential apparatus used as a dispersing means. A grating is an interferential apparatus in which we have many waves (more than 100,000 in some of the existing gratings); in the so-called "interferential apparatus" we have much less (sometimes only two), but the difference of path can be very large. When compared to gratings, the "interferential apparatus" is a simplification. Its use in spectroscopy is very old; Newton used his *rings* for the first measurement of wave-length and sixty years ago Fizeau did very remarkable work with them. But Michelson must be credited for having introduced their use by his work of outstanding importance where he used interference in its most beautiful simplicity, in the phenomena of the interference of two single waves. Then many other kinds of apparatus have been devised in which more than two waves are used, constituting methods intermediate between the two-wave apparatus and the grating.

The interference apparatus, without superseding prisms or gratings, can be properly used for two kinds of problems.

First, they allow very precise measurement of wave-length, either by direct comparison with the meter or by comparison between two wave-lengths.

Second, their resolving power can be increased without limit, as far as the constitution of the light used permits. By this means Michelson has discovered the curious constitution of many spectroscopic lines, consisting of a principal line with several "satellites" so near that prisms or gratings could not separate them.

Since Michelson was searching for perfectly monochromatic light, I suppose that this discovery was a great disappointment to him. Much experimental work has been done to separate the satellites, measure their positions and find the laws governing their distribution. Nagaoka and his associates in Japan must be credited for a great part of the results obtained for which interference apparatus have been almost exclusively used.

The interpretation of these curious "satellites" has been attempted in two different ways. Sommerfeld attempted it by the theories of relativity where the Keplerian laws are only a first approximation (as shown by the motions of the perihelion of mercury); he succeeded in explaining the "duplicity" of the lines of hydrogen in the Balmer series and of ionized helium. In the more complicated case (mercury, bismuth, etc.), Nagaoka attempted to correlate the complexity of the lines with the existence of several isotopes. It is seen here that the most modern theories of physics become involved in this study of the "fine structure" of the spectral lines.

Crystals.—The fourth resolving apparatus is the crystal, the only means which can be used for X-rays. It will be sufficient if I merely mention this, for the sake of completeness, as the two leaders in the matter of X-ray analysis are at present in this city.

As has been shown, we are very far from having at hand the ideal resolving apparatus. At least, using all the existing apparatus, do we find in every case what we want? Unfortunately, very far from it. In many cases, we are handicapped by the lack of transparency, by the lack of resolving power, of light-gathering power, etc. In one part of the spectrum, between the extreme ultra-violet and X-rays from 12 to 100 Å., a resolving apparatus is missing entirely, and we do not have any other means of separating the radiation than the unequal absorption of materials. Only very rough measurements of wave-length have been made with the use of the diffraction pattern produced by a single slit, by Holweck.

RECEIVING APPARATUS.

Coming to the receiving apparatus, we are again very far from finding the *ideal,* whose extraordinary qualities I will not enumerate so that I may not increase our regrets at not having it.

The eye is a very remarkable receiver for a small part of the spectrum. It acts very rapidly (it is saturated in 1/10 of one second) and in the best region of the spectrum it can detect an energy flux of 10^{-9} erg: sec. or 10^{-16} watt. Unfortunately, this sensibility cannot be increased in increasing the time of action (that is the reason for its rapidity) and its sensitivity is confined to a very short part of our field. The importance of the eye as a direct receiving apparatus in spectroscopy is at present very small.

The photographic plate is probably the most important receiving apparatus in modern spectroscopy. Its field of sensitivity, making some changes in the process of preparation, extends from the limit of X-rays to the beginning of infra-red, that is from perhaps $\frac{1}{100}$ Ångström to 10,000 Ångströms. One is most impressed by the advantages of a good thing when it is missing; that is the case for photographic plates when we go to study the infra-red region.

With the question of the receiver is connected the question of the measurement of intensities of radiation. The only fair definition of intensity of a radiation is by means of the energy flux which it carries; we do not have any other means of measuring it than wasting that energy by transforming it into heat; the thermometric receiver is the only universal receiving apparatus and the only direct measuring apparatus for intensities. Unfortunately, in spite of very important and difficult work, the thermometric apparatus is very imperfect in respect to sensitivity and resolving power. That explains why in the infra-red part of the spectrum, where the thermometric receiver has no substitute, our knowledge is relatively small, and why the measurement of intensities is entirely missing in a great part of the spectrum.

SOURCE OF RADIATION.

For the astrophysicists, the question of source of light does not exist; they have nothing else to do than to gather as much light as possible. The first laboratory spectroscopists did almost the same thing, using the simplest sources of light without much

effort to improve them. The conditions are now very different. One of the chief problems in spectroscopy is the investigation of all the radiations an atom can emit; the great variety of means necessary to obtain all the possible radiations from an atom, either neutral or ionized, has been long recognized. Development of the sources of light is one of the main necessities in a spectroscopic laboratory.

The purely thermic radiation is that of a solid body, and especially of the black body, whose theoretical importance is very great; the possibility of having such a purely thermic radiation for a gas has long been discussed; the thing can no more be contested, and that leads to the development of the electric furnace, in which we can have at a high temperature, a volume of a vapor (*in vacuo* or in any gas) in conditions exactly under the control of the physicist.

Flames are the most ancient means of producing light from a vapor; the radiation originates, probably for the most part, from a thermic process; the process is very simple, but cannot easily be controlled.

At present, however, the most important results are obtained with electrical excitation. The recent improvements have been in two directions:

First, the excitation was made in very definite and controllable conditions. That leads to producing excitation by means of electrons with an exactly known and controllable velocity, in a pure gas at low pressure. Important results have been obtained in measuring the energy necessary to produce the different radiations.

Secondly, one aims to have more and more powerful excitation. The difficulty is not in having the necessary electrical means; the electrician offers all we want, including voltage as high as one million volts; the difficulty is to actually use the energy in producing the radiation. As recent improvement, we must mention the experiment of Anderson on "exploded wires" and the beautiful work of Millikan on the extreme ultra-violet radiation with high tension spark in high vacuum.

Very recently, a new process of electrical excitation has been discovered by one of my students, M. Morand, using a beam of positive rays stopped by a material obstacle, he succeeded in obtaining the lines of ionized lithium which had not been obtained before.

NUMERICAL DATA IN SPECTROSCOPY.

I should like to conclude with a rapid recount of the present status of the numerical data of spectroscopy. No science, perhaps, has more important numerical material than spectroscopy. I do not have exact statistics on the number of spectral lines recorded in laboratory or astrophysics; I suppose a hundred thousand is a conservative estimate. Moreover, wave-lengths are measurable with very great precision, in many cases better than one part in one million.

I believe the spectroscopists have been very wise in managing this important stock, improving more and more the precision, changing the standard lines when it was necessary without destroying the results formerly obtained. A little historical survey is necessary to well understand the present status.

We must go back as far as the time of Newton to find the first measurement of wave-length. The radiations were defined only by the names of colors; the measurement was as precise as it could be with such rough specifications. The importance of those measurements was not well understood, even perhaps by Newton himself. It was completely understood only after the foundation of the wave theory by Young and Fresnel, at the beginning of the nineteenth century. At that time, the great task was to show the importance of a periodical phenomenon in optics, and not to have precise measurements; Fresnel measured the wave-length of the red line with his famous " two mirrors," found the same result as Newton obtained by another process, and said it was a good check for his theory. For the other colors, he just copied the figures of Newton.

Precise measurement began with Fraunhofer in 1823. Fraunhofer built the first gratings and used them for the measurement of wave-length of the chief lines of the solar spectrum (discovered twenty years before), and some lines of flames. His results already had a very good precision. In the relative values of the wave-length the error does not exceed 1/10,000 and the absolute values (expressed in inches of Paris, a unit already obsolete in France at that time) have an error of 1/1000.

As you see, we must already distinguish relative precision from absolute precision. The measurement of ratio of wave-lengths does not require anything but a measurement of angles, without any reference to a material standard of length; the abso-

lute measurement of wave-length is a problem of metrology as well as of optics, and the improvement of such measurements is connected with the progress of metrology. The spectroscopists had the liberty to entirely disregard this problem as altogether out of their field, as some Americans wish to do as regards European problems. It would be sufficient for that, to choose as a unit a defined wave-length, and to measure the others by comparison with that; they have not done this, and I think it has been a good thing, because their allegiance to the metrologists has been a good thing for both.

During the thirty-five years after the great work of Fraunhofer, many new facts concerning radiations have been discovered; they remained somewhat scattered until the famous work by Kirchoff and Bunsen (1860); this work did not give many new facts, but it united what was scattered and forced the attention by means of important applications.

The necessity for distinguishing every radiation by a figure became evident. From this point of view, the work of Kirchoff and Bunsen shows a real step backward, if we compare it with the work of Fraunhofer. They defined the radiations not by their natural values, the wave-length, but on an arbitrary scale connected with the deviation by a prism. The progress of modern spectroscopy would have been impossible if such a process had been continued.

Fortunately, the retrogression was short. In 1864, Ångström resumed the work of Fraunhofer by the same methods, with much more elaborate apparatus and very good precision. A little later the work was continued by Mascart and by Cornu in the ultra-violet.

It became the practice to measure every new line by interpolation with known lines used as standards, thus forming a system of wave-lengths. For twenty years almost all spectroscopic work was done, using the figures of Ångström. The precision was pretty good; the absolute values are in error of 2/10,000 on account of an error in the metrologic part of the problem; in the ratio no error arises greater than a few parts in 100,000.

Meanwhile, the resolving apparatus became more and more powerful, and the interpolations became more precise than the standard wave-length used for them. The necessity of having

new standard figures was strongly felt. This work was undertaken by Rowland, with his concave grating and the use of his "coincidence method" without any divided circle. In 1893 he published the wave-lengths of a series of lines, and some years later the wave-lengths of all the lines (about 20,000) which he could detect in the solar spectrum with his grating. Rowland thought the ratios were exact to 1/1,000,000, and the absolute value to 1/100,000.

The work of Rowland was a very great progress, and his results were immediately adopted as standards by all the spectroscopists, abandoning the figures of Ångström.

This work was not finished when new progress was made in spectroscopy, especially through the work by Michelson. This progress arose first from an investigation of the method of producing exactly monochromatic light, and secondly, from the return to interferential apparatus, simpler than the gratings. In 1893 Michelson, with the help of the late Benoit in the Bureau International des Poids et Mesures, determined the wave-length of the Cd red line with a precision leaving far behind all that had been done previously.

The comparison with Rowland's results showed for the latter an error of 1/30,000. The spectroscopists, at first, did not pay attention to this error, thinking that it was just a little difference in unit, and that it would be sufficient to correct by a correcting factor, all data, expressed in the Rowland system, to have perfectly correct results in metric unit.

It was in order to determine this "factor of reduction" that together with M. Perot I made in 1900, a series of comparisons between the red cadmium line and the solar lines. The comparison was done by a new interferential process, and it was done on the solar lines because Rowland had directly measured these lines, measuring arc lines only occasionally and perhaps with less care. The unexpected result was: The "factor of reduction" was not constant; small systematic errors existed in the measurement of Rowland; in the ratios, errors existed as great as 1/100,000.

The necessity for a new system of wave-lengths was soon recognized. I did this work in 1907 with M. Buisson, and the same measurements were made by our method in several other laboratories. From these measurements, which agreed very well, developed what is called the International system of wave-lengths,

whose precision is probably, as well in absolute as in relative values, one part in one million. It covers all the visible and a part of the ultra-violet; it would be desirable, but not easy at all, to extend it in both directions.

The importance of precise measurements of wave-lengths has been considerably increased since the discovery of numerical relations between the wave-lengths of the lines of one spectrum or of different spectra. This discovery has been one of the most important features of modern physics. After many fruitless trials, the way was opened in 1885 by Balmer, who discovered the law of the series of lines of hydrogen. He was, as many others, seeking for simple ratios between the wave-lengths of different lines; he found commensurable ratios, not very simple, but it was correct with a precision of one part in 100,000, excluding all possibility of fortuitous verification. The ratios were such strange fractions as $\frac{26}{20}, \frac{189}{125}, \frac{8}{5}$. Finally, he arrived at this celebrated formula, giving the wave-length of the different lines

$$\lambda = K \frac{m^2}{m^2 - 4}$$

K is a constant, and m a whole number, 3 and over.

The discovery of Balmer and some important remarks made a few years before by Hartley and by Liveing and Dewar were the starting point of a great series of researches.

We find first the purely empirical work by Rydberg, Kayser and Runge and many others on line spectra, of Deslanders on band spectra, an always increasing number of lines in the not too complicated spectra being classified in regular series.

Afterward came many trials to explain the observed laws with ordinary mechanics and electrodynamics; they met with a very moderate success.

Finally, in the most modern period, one is obliged to give up the classical electrodynamics, or at least to modify it by the introduction of the strange quanta rule. In the inner part of the atom the phenomena producing the radiation do not follow the same laws we find outside it. The entire history of the atom, said Ritz about fifteen years ago, is written in its spectrum, but it is written in hieroglyphics which we cannot decipher. We are now on the way to deciphering them, and what we are able to read

is much more extraordinary than any history written on the old Egyptian monuments.

For that purpose, measurements on the whole spectrum are necessary, including that part where we have at present no measurements at all. Moreover, very precise measurements are useful, because the laws recently found are not approximate laws; the verification becomes better when the precision of the measurement is increased, and that is the character of exact laws (while approximate laws vanish when the precision is increased). In as complicated spectra as iron spectrum, recently laws have been found whose verification holds with a precision nearer than one part in one million (there are not many laws in physics which can stand such a severe test) and that is an excellent check of the measurements.

During some time one could have doubts about the usefulness of recording the many lines of the different spectra and of measuring them with the greatest precision. Rarely, precise measurement, although sometimes tedious, is entirely fruitless; in this case it has proved exceptionally important; the experimental work is far from being finished; but now it is done with a perfectly definite purpose; it is actively pushed, and without any doubt, it will lead to very important results.

An Automatic and Continuous Recording Balance. J. R. H. COUTTS, E. M. CROWTHER, B. A. KEEN and SVEN ODÉN. (*Proc. Royal Soc.*, A 735.)—From one pan of a balance is suspended a permanent magnet consisting of a hollow cylinder of cobalt magnet steel. Its axis is vertical and it hangs in a solenoid supported independently so that its lower end is about midway between the upper and the lower ends of the coil. A storage battery furnishes a current that divides at a point. One part goes through the solenoid and thence to a sliding contact moving along two wires. When it has traversed the contact to the second wire it is rejoined there by the second part of the current. The undivided current then returns to the cell. Suppose the weight on the balance pan without the magnet to increase. The beam tilts and contact is made so that an auxiliary current moves the sliding contact in the direction so as to strengthen the current through the solenoid and thus to draw down its pan until balance is restored. The position of the slider is automatically recorded, and the connection between its position and the pull exerted by the solenoid is determined by calibration. When the slider reaches the end of its possible motion another mechanism discharges a metal ball upon the balance pan that needs additional weight and the slider

is moved back to its position corresponding to minimum current in the solenoid. The chart has a curve showing the position of the slider plotted as abscissa against time as ordinate. At a certain instant this curve ceases, when the ball is added, and a second curve begins simultaneously and consequently on the same time level as the first.

The instrument owes its origin to the Soil Physics Department of the Rothamstead Experimental Station, whose distinguished director, Sir John Russell, recently attended the British Association meeting in Toronto. One of the problems it solves is this. The pan without the magnet is suspended near the bottom of a vessel containing a dilute suspension of clay in a weak acid solution. As flocculation and sedimentation proceed the increase of weight on the pan is recorded showing three stages: (*a*) No deposit at all, (*b*) sedimentation at a uniform rate, and (*c*) slow sedimentation of the incompletely flocculated material.

This instrument cannot but prove a valuable addition to apparatus for research. G. F. S.

The Variation of Young's Modulus at High Temperatures. C. H. LEES, J. P. ANDREWS and L. S. SHAVE. (*Proc. Phys. Soc.*, Vol. 36, Pt. 5, 1924.)—Rods or wires about five centimetres in length, supported in a horizontal position, were heated within an electric furnace and the depression of the free ends was read by a microscope. From the change in the depression the change in the modulus was calculated. The modulus of nickel sank from 17.6×10^{11} at 15° C. to 12.2×10^{11} at 500° C. In the case of aluminum the change is even greater. At 495° the modulus has a value less than one-third as great as at 12° C. Nickel, aluminum and platinum all show a slow decrease of the modulus up to an absolute temperature equal to about one-half of the absolute temperature of the melting point. Beyond this the decrease becomes more rapid as the temperature goes up. On the other hand, the modulus of fused silica rises slowly up to about 700° C., after which point it decreases again.

G. F. S.

On the Absorption and Scattering of γ-Rays. N. AHMAD and E. C. STONER. (*Proc. Royal Soc.*, A 735.)—The total atomic absorption of γ-rays derived from RaB + C and filtered through lead is examined and found to be the sum of two terms, one of which is proportional to the cube of the wave-length of the radiation. This seems to represent true absorption. The second term, a constant times the product of the atomic number of the absorber by the absorption per electron, gives the scattering absorption. The most interesting product of the discussion is the fixing of .019 Å.U. as the upper limit of the wave-length of the rays. This means that the γ-rays have a length of wave approximately 1/300,000 of that of sodium light. G. F. S.

THE PROGRESS AND PROMISE OF ENGINEERING.*

BY

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IT IS quite universally conceded that man has made progress toward what we are pleased to call civilization only as he has developed tools and processes wherewith he has been able to more fully control his environment and thus more fully satisfy his material and mental wants. Even the ancient Hebrews, when invading Palestine under the direction of Jehovah, admitted that they could not drive out certain native inhabitants "because they had chariots of iron." The situation is in no wise changed to-day. To that people which possesses superior chariots of iron, always goes the greatest control of their environment. We acknowledge these general principles when we speak of the Stone Age, the Age of Bronze, the Age of Iron, and the Age of Steel and some of us look forward to a still more highly developed age, the metallic index of which we can, as yet, only dimly realize.

It is customary to ascribe man's superiority in controlling his environment as compared to other members of the animal kingdom to the fact that he is a "tool-using animal." This is true, of course, but not sufficient. So long as man was compelled to depend upon tools actually manipulated by his hand, his progress was quite definitely limited. But man long ago found ways and means of harnessing the powers of nature to assist him in his work. He is, therefore, also a "tool-controlling" animal, and modern tools of production driven by modern power plants and *guided* for the most part by the human hand have, to a large extent, relegated actual hand work to a secondary position in the industrial field.

To these accomplishments must be added a third, namely, man's development of ways and means of transmitting intelligence. The problem and need of communicating over long distances is as old as man himself. The solution of this problem as represented by the telegraph, telephone and radio systems of the United States is a thing to marvel at for its perfection.

* Address delivered Wednesday, September 17, 1924, on the occasion of the observance of the centenary of The Franklin Institute.

Until comparatively recent times, discovery or invention and application of discoveries or inventions in these lines of progress were most frequently conducted by the same persons and usually in the atmosphere of and in close connection with actual production. But to-day, probably because of the fundamental law of division of labor, there is an increasing tendency to separate discovery and application. Thus we speak of the field of pure science and of applied science as though they were unrelated. It should be remembered that knowledge, scientific or otherwise, is useless unless it can be applied to human problems of some kind. A learned man who does not use his knowledge to better in some way the sum of human happiness is merely an object of curiosity. The most important medical discovery is useless until it can be applied. The field of work wherein scientific knowledge is applied to industrial problems has become known as the field of engineering. The tendency to broaden this definition in recent years has been quite marked. Used originally to designate the design, construction, and operation of industrial works, it has been extended to cover practically everything in the way of industrial work, including the problems of humanity so far as they are affected by modern industrial methods.

It would be superfluous to recount the accomplishments of applied science as represented by the field of engineering. In transportation and communication, it has almost eliminated time and space and made the whole world neighbors. It has given progressive nations physical comforts undreamed of by our ancestors and made possible a dissemination of knowledge heretofore unknown. I am not unmindful of the great progress that has been made in other fields of applied science, such as medicine and agriculture, nor of the hope that progress in these fields holds out to humanity. But, nevertheless, engineering is the great outstanding feature of modern civilization and all other fields of applied science depend, perhaps, more than is usually realized, upon engineering. The development of modern printing processes, which are almost purely engineering in their scope, has done more to disseminate knowledge in all branches of learning and has, in fact, contributed more to civilization than any other development excepting only the steam engine. It is, indeed, the Age of Steel.

But more important than these physical comforts is the man-

ner in which modern applied science has enlarged our vision of humanity and its problems and the consequent effect upon our ideals of life on this planet. It may be logically questioned as to whether man has made *intellectual* progress since the golden days of Athens, but certainly we have widened our ideals. Or rather we have admitted among our ideals possibilities, that, while recognized as desirable by our progenitors, were pushed aside because of expediency. All history indicates that men's ideals have always been limited and modified by the problems of making a living and in general as the tools of production have been improved and living conditions made consequently easier, more tolerant ideals have usually prevailed. The ancient Greeks with all their intellectual development knew of no way in which to support their intellectual life except by slavery. We, in an age when productive methods are immensely more highly developed and in a land where natural resources are ample, hold slavery to be not only unnecessary, but actually unmoral. The ideals of all ancient civilization were the *ideals of necessity*. To a certain extent this is still true of our civilization. But for the first time in the history of the race, there is held out a hope that through modern methods of production, we may attain an ideal of existence that has long occupied man's mind, but because of necessity has remained a Utopian dream. If I read the ideals of American democracy aright, we are committed in this country to an effort to attain universal well-being, where every man, woman and child shall have enough to eat, drink and wear, proper housing, and some measure, at least, of mental and spiritual development. And so far as can be seen, the only hope that this can be accomplished, lies in applied science; and this is the principal reason for the faith that is in us who believe in the development of science, pure and applied, to the uttermost.

To those of us who have this faith and confidence in science and its application, it is sometimes painful to reflect that millions of people in this country and elsewhere are not only ignorant of the scientific method of thought, but have also little or no faith in the ultimate results of the application of science. When Mr. Bryan rails at the theory of evolution, we are amazed and cannot understand how a man of such apparent intelligence can doubt the evidence that to the scientific mind is so convincing. And we are prone to dismiss the entire matter by classifying Mr. Bryan

and his kind with the ignorant, or at least with those whose fundamental education has been neglected. It may be worth while to glance at a few of the objections to science and its applications and, if possible, answer them.

Of course, as concerns those who do not understand the field of pure and applied science, and consequently have no faith in them, the greatest hope lies in educational methods. Science, after all, is simply classified knowledge and its basic conceptions are open to all who are willing to read. An excellent presentation of this problem, including the fundamental reasons for disbelief in science, will be found in the little book issued recently by James Harvey Robinson, entitled "The Humanizing of Knowledge." Universal recognition of scientific methods of attacking all problems can be brought about only through education in basic principles. When this shall be accomplished, we shall make more rapid progress in all industrial, social and political problems.

But there are not wanting critics who, while even having knowledge and sympathy with pure and applied science, often express a fear that we shall not be able to continue to support a civilization even as high as the present one, as natural sources diminish and population increases. To those unacquainted with modern science, pure and applied, this may appear to be a reasonable fear. But if the scientist can give the engineer two more important discoveries, namely, an unlimited supply of power from the energy of the universe, and a method of producing food synthetically, there is no reason to doubt that the present era of comfort can be continued and progress yet undreamed of can be made. Of course, to the average person, the possibilities of such discoveries may seem visionary, yet, when compared with scientific achievements already attained, they are no more visionary than were the flying machine, the telephone and the radio of a few years ago. I firmly believe these discoveries will come to pass.

These critics again express the fear that the tools of destruction now being produced by the scientist and the engineer will, in the near future, blot out civilization because of their terrible death-dealing possibilities. I have an idea that the first stone axe was viewed with much the same alarm and the invention of gunpowder certainly must have raised the same fears. Yet the race has increased and prospered, protective and defensive measures keeping close pace with destructive tendencies. The writer,

of course, does not defend in any way the use of science for destructive purposes, but he does not share the fears of those who think we shall finally be obliterated by our own weapons.

The third, and to my mind the most important criticism of modern civilization, is that while our present industrial methods have undoubtedly bestowed many physical comforts upon us, we are, on the whole, little better off than our forefathers, and that the evils of modern industry more than offset the benefits derived from it. It is pointed out that the distribution of wealth is still unfair, that we apparently are not able to direct the great energies we have developed to our best advantage, and that justice, whatever that may be, is still as remote a virtue as it was when Plato described it as the essence of all good things. A multitude of reasons are advanced for the dissatisfaction and discontent of modern life, many of them having little or no bearing upon the real roots of the problem. But from the general tenor of the suggested remedies, it is obvious that many, if not most, of our troubles have their roots in the industrial system under which we live—as might be expected since industry is the backbone of all existence. An examination of industrial laws preceding the present era and going back into remote antiquity, indicates that modern industry has introduced few if any new problems. It has, however, accented and made more acute the industrial problems that always arise where congregated industry and division of labor are factors in production.

Briefly and without detail, the most important changes wrought by modern engineering methods upon industrial life are as follows: The separation of agriculture and mechanical arts that began hundreds of years ago has completed and widened. The agriculturist now depends upon the engineer for his tools of production. Within the mechanical arts group the actual worker has been separated almost entirely from the ownership of the tools of industry. A complex system of transportation independent of either of these older fields of occupation has been developed. Communication by telegraph, telephone and radio has made all men neighbors and has accelerated our industrial machinery to a marvellous degree. And lastly, both in transportation and the mechanical arts, division of labor has been extended to a degree unheard of by our ancestors and made possible only by the use of modern tools of industry. The resulting complexity of human

relations coupled with the vast increase in population has accented the time-old problem of "what is mine and what is thine," to a point where many people despair of a solution and advocate a return to simpler methods and consequent simpler relations. There are those, indeed, who openly assert that modern industrial life is a curse and not a blessing.

The charge that modern civilization is a failure or, at least, no improvement upon former civilization, should be of peculiar interest to the engineer, using this term in its widest sense to include all industrial workers who use the scientific method. For modern civilization is largely what he has made it, and the civilization of the future will be largely what he wishes it to be. And few, if any, engineers will agree that the solution of our troubles is to be found by turning back the clock. To move backward is to accept the doctrine of despair and thither lies poverty and degradation for all but the favored few. The way lies forward and the hope of universal well-being rests in applying to these difficult social, political and industrial problems the same method of attack that has enabled the scientist and the engineer to make such marked progress in subduing our environment.

In fact, the greatest contribution that modern science has conferred upon us has been to instil firmly into our minds, the idea of *progress*. The philosophic outlook of the eighteenth century held before men the idea of a Utopia of the past and taught the desirability of restoring conditions as they existed centuries ago. It taught that the men of olden times were wiser than we moderns and hence should be followed without question. The scientific and engineering developments of the nineteenth century changed all this and held out to men the hope of a better civilization than has yet existed because these new, yet old, methods of thought and action gave man a greater control over his environment and consequent higher ideals of existence. We are not going backward until we have demonstrated fully that this theory is incorrect.

It may be of interest, therefore, to glance briefly at some of the results that applied science has brought about in industry and other fields simply to indicate the trend of modern development.

From time immemorial, it has been believed that administrative skill and knowledge were elusive personal attributes that were congenital and not easily acquired. To-day, we know that while personality will always be a large factor in successful administra-

tion, there is a well-defined scientific background to industrial organization and management. And this realization has come to us very largely because the engineer has applied his well-known methods of scientific analysis to these fields of endeavor which for countless centuries have been the abode of speculation and empiricism. As a consequence, the field of industrial management is rapidly passing into the control of the engineer and the time will come when only those who are trained in the science of industrial management will be competent to direct industry just as, to-day, none but trained engineers are competent to direct many productive processes.

More slowly, but surely, the field of business is yielding to the same influences. Here again is a great field where empiricism has always held sway. But to-day, we see great universities establishing elaborate and costly schools where the science of business may be taught and more noteworthy, perhaps, is the establishment of such institutions as the National Industrial Conference Board for the express purpose of conducting researches in the problem of business and industry.

Without doubt, these new methods will also greatly change our conception of law.[1] Much of our present law depends for its strength upon precedents established long ago under a different industrial system and does not apply to modern conditions. The scientific method, furthermore, has given us new views in regard to rational methods of attacking problems, which make older legal methods appear foolishly inadequate. We would not think of calling upon any one but expert engineers to decide, say, whether a bridge was safe or not, but we solemnly impanel a jury of men from the street to tell whether a given man is, or is not, insane, regardless of the fact that this is a much more delicate scientific problem, and regardless of the fact that those selected may never have given a thought to the phenomenon of insanity.

And these remarks hold equally true for the larger and more important problems of government. The majority of the problems of city government and a large part of those that concern state and national government are of an industrial character, or of some other type that will yield to rational analysis by those competent to attack them. Yet, our government is not based upon

[1] "The Common Law and the Idea of Progress," by Prof. E. F. Albertsworth, *Jour. Am. Bar Asso.*, July, 1924.

this theory. Plato, speaking of the citizens of Athens, remarks that while every man resented suggestions or criticisms of his handiwork by those not of his calling, every man also believed himself competent to speak as an oracle on the problem of government. We still bask blissfully in the same self-conceit and elect men to important political offices, not on the basis of competency, but of political expediency. If our present form of government fails, it will not be for lack of democracy, as some fear, but because of lack of special knowledge and common honesty. What we need most to-day is more intelligent leadership and less government by popular votes.

Herein lies a greater opportunity for the engineer. It will not be sufficient for him to plan, build and operate the tools and facilities of industry. He must take a greater and an increasing interest in the social and political problems that his handiwork has created and is creating. He alone has an intimate knowledge of the field of modern industry and he should, therefore, be prepared to speak authoritatively of the human and financial problems involved. The basis of all *justice* is *knowledge* and human progress must rest to a large extent upon a knowledge of the experiences of the race. This is true in all lines of human activity and particularly true of a civilization that rests upon scientific achievement. The hope of the future lies largely in making accessible to all, the great mass of accumulated experience that has come down to us. The scientist and the engineer of all men stand out preëminently in this respect among those who know and use classified knowledge to the fullest extent of their ability. The great leaders of the past had, for the most part, no conception of the importance of recorded experience. With few exceptions, they took their knowledge and experience with them when they departed this life. The engineer has been the first to reduce to written form the basic principles of industrial leadership just as the lawyer long ago began to record legal experience, and the work of the engineer in this field has already commanded world-wide attention.

No one would presume to say that the engineer alone can solve all the problems of our complex industrial life. That will require the best intellects to be found in all walks of life and possibly a change in some of our ethical values. But if the engineer will bring his knowledge and his methods to bear upon the problems

of distributing the fruits of industry with the same vigor with which he has attacked the problem of subduing his environment, he certainly can make a great contribution to humanity and do much to make universal well-being more of a reality and less of a Utopian dream.

There is, in fact, but one real reason for doubting the possibility of constant progress in modern civilization and that is based upon the so-called *Law of Diminishing Returns.* This law affirms that all economic gains are accompanied by corresponding losses that tend constantly to equalize these gains. All experience with economic developments in agriculture and manufacturing indicates the universal truth of this statement. And the operation of this law is not confined to the physical side of civilization, but affects the personnel of industry as well. Apparently, no economic gains are possible in industry without affecting the status of some class of worker for the worse, though, *in the long run,* all men may possibly be benefited by the change. The *immediate* and *ultimate* effects of the automobile form an excellent illustration of this point. We are not sure, as yet, whether it will prove a net gain or a net loss to humanity. It may be, therefore, that the increasing complexity of one civilization which tends constantly to offset the economic gains of modern methods may, in time, fully nullify these gains and make further progress impossible. It may be pertinent to inquire whether this law has operated to halt and finally destroy former civilizations as highly developed in some respects as ours. Is this law the seed of destruction that our civilization carries in its bosom? It may be, but we who believe in the efficacy of pure and applied science will not be convinced that it is until the scientific method has been applied much more widely than it is at present to our social and political problems. We are probably a long way from the point of nullification, if we can make progress in the application of rational thinking.

But if it be granted that science holds out the hope of a higher and better civilization, it does not follow that such a civilization will be evolved simply because scientists discover, and engineers make application of, scientific truths. We shall need to know much more than we do now concerning the effect of such application upon our social and political fabric. At present, we are blundering along making scientific applications experimentally and

depending upon the slow process of law to correct the errors; and our experience, so far, has not been altogether reassuring. We are sadly in need of basic knowledge which would enable us to see in advance what the effects of scientific applications may be, socially and politically, and govern ourselves as we do in making scientific applications to the physical side of industry.

And in all this work, the ancient philosophies may help us. An old Greek legend tells how Prometheus was sent to distribute the arts of life among men, giving skill in each only to a favored few. But these arts not proving sufficient to keep men alive in their struggle with each other, Zeus afterwards sent Hermes to them with *Justice* and the *Sense of Shame*, ordering him not to impart them to a few only but to spread them broadcast among men. For without a portion of these, he said, in the heart of every man, human society cannot hold together.

This philosophy would still appear to apply. Modern industry has wrought many injustices and brought about many results of which we should be heartily ashamed. Perhaps what we most need is a modern Hermes to redistribute these ancient virtues in the hearts of all men. We probably, even now, have sufficient control of our environment to raise, considerably, the status of life in all progressive countries. Possibly we lack only the idealism necessary to make a long step forward.

Instantaneous Illumination at High Speeds. G. E. BAIRSTO. (*Proc. Physical Soc.*, Vol. 36, Pt. 5, 1924.)—A way was sought to illuminate model airplane propellers running at the rate of 10,000 r.p.m. so that instantaneous photographs could be taken. In the occurrence of the illumination there could not be an error of more than four-millionths second and the duration was to be no more than half of this length of time. A contact breaker failed to give satisfactory results. Recourse was then had to an arrangement that included transformer, thermionic valve, revolving discharge point, etc., and the ends desired were obtained. Light was caught on a bright point fixed on a disc and was reflected to a photographic plate. From the elongation of the image of the point when the disc was making 10,000 r.p.m., it was found that the sum of the duration of the spark and of its departure from exact synchronism in appearing was .55 millionth second. A silk thread fastened on a propeller blade was photographed at rest and again when illuminated by the method devised with the blade making 11,000 r.p.m. The latter photograph is but little thicker than the former. G. F. S.

THE TREND IN PHYSICAL METALLURGY.*

BY

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METALLURGY through the nineteenth century was a chemical, and, in its larger development, an engineering science. It owes its progress during this period to chemical research for discovering methods of separating associated metals variously contaminated by impurities, and to mechanical ingenuity for devising appropriate economical exploitation of these discoveries. To this century belong the Bessemer and open-hearth processes. Bessemerization of copper matte, electrometallurgical production of aluminum, and various electrolytic refining processes—in short, the great technical advances which have fashioned iron, steel and other metals into a rigid skeleton of all industrial life.

In the early part of the present century came the important flotation developments; but it seems hardly reasonable to suppose that the future can compete with the past in revealing unique and spectacular methods of producing the common metals, unless it be through unprecedented electrometallurgical developments in an age of "Superpower." Normal progress in this field is more likely to yield merely such modifications and improvements as may fit into a changing economic scheme.

With the full development of this older metallurgy comes the birth of a collateral and companion science, which, indeed, fulfills more faithfully than its predecessor the generic meaning of the term, *viz.*, "the working of metal." This later field of the science is dedicated to the general behavior and interrelationships of metals which may be subdivided into constitution, structure, and properties, either from the point of view of pure science, or of subservience to the inevitable industrial contacts which at once emphasize the relationship between properties and applications. No term has by common consent been considered wholly appropriate for specifically defining this field, and, therefore, we may adopt, according to preference, any of the following: The term

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"metallography," which historically, at any rate, emphasizes the microscopic aspect of structure investigations; the term "physical metallurgy," which emphasizes, inferentially at least, the viewpoint of pure science, but is coming rather generally into use; or simply, the "science of metals." This last may be considered by some rather loose phraseology, for it seems to place the metals, *similis simili gaudet,* on a lofty pinnacle of immunity from the tenets of the recognized sciences, but it may also be more indulgently construed as a designation, like the German "Metalkunde," of a definite field of science and technology, growing rapidly under the influence of physical and chemical research.

At the beginning of the present century, our knowledge in this field was chiefly an industrial appreciation of the ordinary useful metals and alloys, based on rule-of-thumb manufacturing methods and on empirical physical or chemical data.

The first trend of scientific exploitation had, however, started in the utilization of the microscope, even as early as 1864, to study the peculiarities of alloy structures. Sorby's early lead was not followed to any extent for some twenty years, but in the closing fifteen or twenty years of the nineteenth century this trend became active, resulting in numerous investigations and an understanding of the more prominent structural characteristics of carbon steel and some other common alloys.

In 1875–1878, J. Willard Gibbs published his famous researches on the "Equilibrium of Heterogeneous Substances," which after a lapse of about two decades gave rise to a new appreciation of the phase relationships possible in alloys and other mixtures, largely through interpretations by H. W. Bakhuis Roozeboom and others. On this foundation were evolved conventional graphical methods of expressing constitutional relationships in two- and three-component systems, as well as an appropriate technique of alloy investigation, and the first two decades of the present century witnessed an active trend of experimental investigation of two- and three-component constitutional diagrams. These combined developments served to give form and substance to the new physical metallurgy, and may be said to constitute the first phase of its existence. We are now on the downward slope of this trend, probably for two reasons.

In the first place, all of the important binary systems have received the attention, independently, of a number of authors, and

the constitution of binary alloys as a class is now a matter of record, for the most part satisfactory and complete. Guertler's "Metallographie," starting in 1912 and continuing at the present time, offers the best critical review of this aspect of physical metallurgy.

In the second place, the tendency toward coördination between theory and practice has increased, largely through the stimulation of war-time necessities which directed the attention of many metallurgists, trained essentially in theory, to practical problems of production and usage. This has forced a realization that the conventional equilibrium diagram, while useful as a background, cannot by any exploitation be drawn upon to explain all of the underlying phenomena upon which the utilitarian existence of metals and alloys depends.

This last point is inherent and was, of course, understood by every well-informed participant in the earlier constitutional studies. It is reiterated merely by way of arguing the trend in physical metallurgy. While there was a dearth of constitutional information in the field of heterogeneous equilibrium, there was widespread eagerness for investigation, doubtless supported by the hope that great discoveries might be made. Scores of laboratories in America and abroad participated in this movement.

The pendulum now swings toward the development of new methods, whereby such things as size, placement, and force relationships governing the interactions between the ultimate or subultimate particles of metal, whether of the same phase or of different phases, may be investigated. This doubtless appears far more alluring to most metallurgists than the preparation of extended contributions to our knowledge of the constitution of three-component or higher order alloy systems, to be gained only by extremely laborious work in examining perhaps hundreds of alloys under a given title. In this connection, it is pertinent to observe that the logical development of the conventional field of constitution of alloys should be *via* the route of training young physical metallurgists. It would be well if all institutions in which metallurgical personnel is developed would contribute some research of this character. The National Physical Laboratory in England, from which a continual stream of accurate and thorough constitutional work in complicated systems has emerged, is a conspicuous example of such activity. The foresight of Doctor

Rosenhain in organizing this work will be appreciated by many investigators of the future, both by way of example and by reason of the specific information thereby made available.

The most marked modern trend in physical metallurgy is toward a comprehensive theory of the constitution of individual phases or phase elements which shall be susceptible to quantitative expression in defining the properties and interrelationships of these phases. From a practical point of view, knowledge of this sort will permit, for example, a specification of the amount and kind of cold work an alloy of given constitution will stand, or the fundamental specification of mechanical properties after operations which have resulted in rearrangement by strain or recrystallization and re-orientation.

The following example may be taken at random to illustrate the present limitations:

White gold alloys, considerably in vogue at present, commonly consist of standard gold alloyed with copper, zinc and nickel. In investigating 18K mixtures, one may proceed systematically by outlining on a ternary copper-zinc-nickel plot the area including alloys which, when added to pure gold in the proportion 6 to 18, will give usefully malleable (18K) white gold of satisfactory color. The process is wholly empirical. The area corresponding to satisfactory malleability does not represent the limiting area of one form of constitution. The alloys in question are all single solid solutions, but there are other concentrations in this solid solution field which would be more satisfactory from the point of view of color if they were sufficiently plastic. This plasticity is highly dependent on purity and may be controlled somewhat by additions of a fifth substance, either for de-oxidation or for some obscure purpose.

How can we intelligently guide investigations of this sort? Certainly not on the sole basis of knowledge as to the identity of the structure-elements present (according to a conventional equilibrium diagram), although such knowledge is essential. A very thorough knowledge of the innermost constitution of the particular structure-elements whose properties we desire to utilize is required, and we stand to-day on the threshold of satisfying accomplishment in this branch of physical metallurgy.

Others, notably Rosenhain in the 1923 lecture before the American Institute of Mining and Metallurgical Engineers, and

Jeffries in the 1924 lecture, have discussed methods and possibilities in this direction. It is desirable that this trend of development be emphasized on every possible occasion before metallurgists at large, so that the constructive research begun by physicists who are responsible for the newer technique of investigation shall be built upon, so far as possible, by a large group of investigators whose general familiarity with metals is vitalized by a life-time of association.

To return briefly to the research problem mentioned above: Other atoms substituted for gold atoms offer resistance to gliding along the planes of dense atom packing in the face-centred cubic gold lattice; but in varying degree per unit of concentration, depending upon the characteristics of the atom introduced and upon its effect on the lattice. These characteristics and effects are known only approximately. Rosenhain pointed out in his 1923 lecture that "the hardening effect of one metal on another in the form of a solid solution is closely and inversely related to its limiting solubility," due to the distortion of the lattice of the parent metal to the point of instability in all cases where maximum solid solubility is attained, whether this maximum solubility be great or small.

On this basis, one might develop a plasticity or malleability diagram from primary consideration of the solid solubilities of the various metals, nickel, zinc, etc., in gold; but it must be admitted that the cold working characteristics, or other specific mechanical behavior, of saturated solid solutions of two different metals in the parent metal would differ in accordance with certain atomic characteristics of the metals involved, and with certain considerations bearing upon the concentrations and placement of the atoms.

Any argument along these lines would soon lead to the conclusion that we are at present in a qualitative stage in the correlating of the various useful properties of alloys with their constitution. It would therefore appear that progress in the immediate future will be largely in the direction of accumulating, in precise form, data on the constitution of single-phase crystalline bodies and of attaining a precise characterization of the possibilities of structural variation under various technological conditions.

The latter type of research has been actively prosecuted for several years, perhaps most prominently by Polanyi and his

associates,[1] who have discovered that rolling, wire drawing, etc., lead to preferred orientations in the final crystalline aggregate. Much use might be made of this type of investigation in modifying existing practices of metal working in the direction of developing the best properties to meet given conditions. The modification would appear particularly pertinent in the case of metals, such as zinc, which are of lower symmetry in their lattice structure than the face-centred cubic metals. Standing in the way of a widespread utilization of these methods are the slow and difficult methods of determining orientation of the units in a crystalline aggregate and the inaccessibility of X-ray diffraction apparatus to the great body of metallurgists.

There is much interest at the present time in the preparation of single crystals and the study of their properties. The recent work of Carpenter and Elam on aluminum [2] with its extension by Taylor and Elam [3] serves as a model of precision in this field.

We may confidently expect that this painstaking study of the behavior of crystals singly and in various measured degrees of association will soon result in a common understanding of the nature of the oft-discussed boundary region between crystals. It would seem that the amorphous cement theory is reaching a critical period of its existence, from which it will either emerge triumphant or give way wholly to another conception of the boundary region.

Physical metallurgy owes a debt of gratitude to physicists, from Laue and the Braggs on, who have developed the theory and practice of X-ray spectrography. More than any other agency, this has carried physical metallurgy into a new era. The character of the apparatus, along with the specialized training needed to operate it and interpret the results, leaves some doubt as to its general adoption in metallurgical laboratories. A comparatively inexpensive design of considerable adaptability in producing the transmission, Debye, Hull, or Bragg type of spectrograms, would be of assistance in stimulating this kind of work. Meanwhile, a comparatively small number of investigators continue to add to our knowledge by individual work in the fields of their choice.

[1] A variety of papers published in *Zeitschrift für Physik* beginning in 1921, notably, "Über Strukturänderungen bei Metallen," **17**, 1923, p. 42.

[2] *Proc. Roy. Soc.*, **100**, 1921–1922, p. 329.

[3] Bakerian Lecture, *Proc. Roy. Soc.*, **102**, 1922–1923, p. 643.

The simplest and most direct use of the X-ray spectrograph in physical metallurgy is the examination of pure metals through a suitable range of temperature in order to determine whether one or several crystalline forms occur in the same metal. It has been necessary in the past to argue these questions of polymorphism purely on the basis of continuity or discontinuity of chosen physical properties through the temperature range in question, although in some cases a pronounced structural change (recrystallization), as in the transformation alpha to gamma iron, gave a touch of finality to the argument. The comparatively recent review of the situation in the light of X-ray researches,[4] whereby an alleged beta iron loses its status as a separate crystalline form, is fresh in the minds of metallurgists. It is difficult to conceive of any disclosure of theory which will in any way disprove the finality of the X-ray spectrograph as used at present in proving the presence or absence of different arrangements of atoms in the space lattice. There may, however, be further advances in determining the particulars of various lattice structures, and questions of allotropy as related to changes within or around the atom are ever before the physicists. Doubtless much of value to physical metallurgy will come through these agencies, but first we shall have a complete survey of the metals with respect to their habits of crystallinity throughout their entire temperature range of existence.

Quite recently Pierce, Anderson, and van Dyck of the Research Department of the New Jersey Zinc Company, have conducted a number of interesting experiments on the alleged allotropy of zinc. The work is not completed, but in its present stage appears to have settled the question at issue. The varieties of zinc used in these experiments were

		Cd	Fe	Pb
(1) Horse Head	—	Trace	0.014	0.03 –0.05
(2) "C.P"	—	None	0.004	0.003
(3) "S.P"	—	Containing no impurities in amounts detectable with the quartz spectrograph.		

In the main experiments, twisted ribbon was used, as this was found to present a maximum number of orientations for analysis by means of the X-ray equipment manufactured by the General Electric Company. The ribbon was heated electrically and exposures were made at the five approximate temperatures: 20° C., 120° C., 225° C., 350° C., and 375° C.

[4] Westgren, *J. Iron and Steel Inst.*, 103, 1921, p. 303.

Twenty-six planar spacings were noted in the aggregate on the films from the twisted ribbon at the five temperatures. Twenty of these could be matched with the spacings calculated or observed by Hull. Whether the others were due to experimental defects or compel a modification of the Hull lattice for zinc is not yet determined. Ten of the Hull spacings were observed at all five temperatures. This in itself strongly indicates an absence of polymorphism, as a changing pattern would hardly preserve some 50 per cent. of the original spacings. Further details of this analysis indicate that the absence of spacings on the films was not characterized by any regularity such as would accompany a change of pattern, but was fortuitous and presumably due to a failure to recognize on every occasion all of the diffractions normally present.

Thus, one of the twenty-seven calculated spacings was missed at each of the temperatures except the highest, although it was observed in a sample of wire at ordinary temperature. Four were missed entirely, although two of these were observed in samples of foil or wire at ordinary temperature. Three were missed only in the first. Two were missed at the first, fourth and fifth temperatures. Another was missed at the third only. One was missed at the first and third. One was observed only at the third temperature, and another only at the first and third, although the latter was found in foil and wire at ordinary temperature.

On the basis of these results, the utmost that could be conceded in the direction of polymorphism would be a possible trifling change in the lattice with the temperature, quite apart from the well-differentiated patterns found in demonstrated cases of allotropy.

The difficulty in appraising the value of earlier researches, comprising measurements of electrical conductivity, thermal or electrochemical e.m.f., hardness and other mechanical properties, is due in part to uncertain purity of the material used in the various tests, and in part to the ease with which zinc changes structurally by recrystallization with an undetermined effect on the property measured. That these, in common with other difficulties, complicate the situation is evidenced by Doctor Rosenhain's conclusion,[5] that "the allotropy of zinc itself has also been very thoroughly studied and the reality of the trans-

[5] "Metallurgy," "New Encyclopædia Britannica," Vol. 2.

formations well established." If allotropy exists, the present X-ray measurements indicate, as noted above, that it cannot be an allotropy of wholly dissimilar atomic arrangement in the space lattice, as, for example, in the case of alpha-gamma iron.

Beginning with the discovery of "slip bands" by Ewing and Rosenhain some twenty-five years ago, the structural changes in metal brought about by plastic deformation, subject to direct observation under the microscope only in their early stages, have occupied the attention of numerous investigators, who have sought in vain for a complete explanation of the process. A single grain becomes transformed into a number of grains by a process of cold working followed by annealing, so that either the working or the annealing gives rise to a variety of orientations, constituting a disintegration of the original crystal.

The author, in association with A. Phillips,[6] has considered fragmentation to be an essential feature of all severe cold deformation processes, the fragments after severe cold working representing tiny displaced parts of the lattice, *i.e.*, newly oriented individuals. In stating that "the form of any curve" representing fragmentation "will be determined by the nature and intensity of the deformation sustained by the metal," we had in mind the probability that the size and distribution of fragments would vary greatly with the nature and direction of the forces causing the break-down. Preferred orientation of fragments was not discussed in this early paper, and in any case would have represented only another stage of imaginative conception.

The X-ray spectrograph in the hands of Bain and Jeffries[7] now dispels any doubt as to the reality of some kind of fragmentation by cold working, and the continuing observations of Polanyi,[8] Jeffries,[9] Pierce, Anderson and van Dyck[10] and others who are examining wire, sheet, etc., by this method, reveal the selectivity of various deformational processes in arranging the fragments to suit the various conditions imposed.

The Debye or Hull method shows the effect of cold working a coarse-grained metal principally by filling out the diffraction

[6] *Trans. Am. Inst. Mining Eng.*, **54**, 1916, p. 608.
[7] *Chem. Met. Eng.*, **25**, 1921, p. 775.
[8] *L. c.*, p. 6.
[9] *L. c.*, p. 5.
[10] Private communication to the Author.

pattern because of an increase in the number of orientations due to fragmentation. The Laue or transmission method, on the other hand, yields a characteristic effect called asterism, which has often been observed, and in general X-ray spectrographic work causes the rejection of certain crystals as defective or unsuitable.

In ordinary sources of reference, I am unable to find a generally accepted underlying explanation to this effect, which is the subject of considerable discussion by Czochralski.[11] He used the Laue method in studying aluminum crystals, arranging these in a variety of positions and combinations, and ultimately introducing the feature of cold deformation. Czochralski observed displacements in the characteristic zone circles of the Laue patterns as deformation progressed. These displacements took the form of a lengthening out of the individual reflection points, and finally a whorled or star-shaped pattern (asterism) made up of many individual reflections. He sought to bring this into harmony with his " Verlagerung," or displacement theory, of cold working, which postulates a distortion of the lattice by atom-to-atom displacement, and may be visualized as a warping or bending of the slip planes. This theory appears to have been argued as extensively in Germany as the amorphous phase theory in England and America.

One may freely admit Czochralski's contention that curvature of the lattice planes would cause a spreading of the separate diffraction spots into more or less distorted ray-shaped patterns, but it is equally satisfactory to imagine a curved lamella broken down into a large number of articulating and separately diffracting elements, or lattice fragments of normal structure.

This latter conception is substantiated by Joffe and Kirpitcheva,[12] whose observations relate to the similar distortion of the Laue pattern (asterism), observed in a compressed crystal of rock salt. I cannot do better than quote from the original paper: " After the limit of elasticity is passed, the separate spots of Laue's röntgenograms begin to elongate. . . . . The detailed photograph of a crystal not too much strained shows in every elongated spot a stratification, consisting of a series of separate spots, the number of which increases with the increase of the strain. Every such beam is at the same time a spectrum, but it is

[11] *Zeits. f. Metalkunde,* 15, pp. 60 and 126.
[12] *Phil. Mag.,* (6), 43, 1921, p. 204.

easy to observe that the stratification is due to separate small crystals into which the whole crystal has parted, and is not a line spectrum of Röntgen rays. The dimensions of the fragments are a few hundredths of a millimetre. On the fluorescing screen and on the photograph, the direction in which the small crystals slide can be seen. The spot corresponding to the rhombical dodecahedron 110 remains completely unaltered, while the others become elongated. The direction of stratification in every beam also shows the direction around which the rotation of the little crystals takes place. Thus we come to the conclusion that a crystal of rock salt when compressed breaks up into small crystals, which, while sliding along the plane of the rhombic dodecahedron, rotate at the same time through different angles."

Thus these authors, in interpreting Laue patterns of strained rock salt, which are similar to Laue spectrograms of strain-hardened metal, reach a conclusion quite in harmony with the views which have been here presented as expressive of the present status of accomplishment in this chapter of physical metallurgy.

I believe that a conception of warped or distorted planes is common in the minds of X-ray investigators as an explanation of broadened diffraction bands or " fuzzy " effects seen in very finely fragmented structures. It is, indeed, most reasonable to suppose that after severe deformation, lattice fragments or lamellæ produced by twisting and sliding along crystallographic planes are in the warped condition of elastic strain,[13] so clearly indicated by corrosion-cracking and other phenomena of internal strain in cold-worked metals.

Until the structural effects of cold working are firmly established and clearly appreciated by the great body of physical metallurgists, progress in explaining the closely allied process of recrystallization will quite likely be restricted to a wholesale gathering of experimental facts and attempts at their classification.

The paper by Mathewson and Phillips, in 1916,[14] stimulated a valuable and rather long discussion of the subject of recrystallization by Jeffries,[15] Howe [16] and others, and the trend since that time, while productive of a mass of data, has not, so far as

[13] *Cf.* Gross, *Zeits. f. Metalkunde*, **16**, 1924, p. 22.
[14] *L. c.*, p. 11.
[15] *Trans. Am. Inst. Mining Eng.*, **54**, 1916, p. 658.
[16] *Trans. Am. Inst. Mining Eng.*, **56**, 1916, pp. 561 and 582.

I can discern, brought us much beyond the speculative stage in appreciating the exact causes of recrystallization or following its very early stages of operation. Jeffries' important contributions on this subject may be conveniently studied in Jeffries and Archer's book, "The Science of Metals," published during the present year.

Carpenter and his associates, in 1920,[17] gave us a beautiful demonstration of grain growth by boundary migration, but after an imposing amount of work on recrystallization of aluminum in this and later papers,[18] he seems disinclined to attempt much in explanation of the observed phenomena, deeming it "wiser to refrain from what can only be speculation."

Quite recently Jeffries[19] has observed that the process of recrystallization in annealing a bundle of very severely cold-worked tungsten wires failed to change the X-ray diffraction pattern of preferred orientations before annealing. We have observed in the Hammond Laboratory that pieces of rolled zinc, annealed so as to produce crystals an inch or so in diameter, invariably bend much more easily around an axis parallel to the direction of rolling than around an axis laid across the strip, thus demonstrating preferred orientation after utter obliteration of the original rolled structure.

It seems inevitable that the intelligent use of the X-ray spectrometer as an instrument for studying orientational changes will shortly reveal in its innermost mechanism the progression from one structural form to another in the processes of cold working and recrystallization, thus giving a quantitative picture of these important and baffling structural reorganizations.

Among the activities which indicate the trend of physical metallurgy, the importance of corrosion research has, of course, been recognized ever since metals came into diversified use. So much data have accumulated that the great present need is to stop indiscriminate experimentation and endeavor by coöperative effort to get at the real elements in an immensely complicated problem. The great professional societies have been devoting attention to this work through special committees. The six reports of the Corrosion Committee of the Institute of Metals of London con-

[17] *J. Inst. Metals*, **24**, 1920, p. 84.
[18] *J. Inst. Metals*, **25**, 1921, p. 259; **29**, 1923, p. 29.
[19] *L. c.*, p. 5.

stitute one of the best sources of information now available. The American Society of Testing Materials has, within a year or two, set up a committee whose activity is directed mainly toward standardizing the form of corrosion testing. The National Research Council's Committee is fostering the type of research directed toward a better theoretical understanding of corrosion processes. Numerous associations of manufacturers or individuals with special interests are active in various directions. The consumer is becoming aware of the fact that the products which he buys can often be improved by more intelligent metallurgical practice. We cannot escape the conclusion that this intense focus of attention from every interested source will inaugurate an era of classification and consolidation of knowledge in this field which will largely dispel the bewilderment experienced by an observer when he seeks now to study or compare materials from the point of view of corrosion resistance.

Close to this field is the field of high temperature utilization of alloys, implying resistance to many forms of corrosion and the maintenance of useful mechanical properties in the heated condition. In the literature of the last twenty years, one might find half a dozen valuable papers dealing with the properties of metal at elevated temperature, conceived and executed largely from the point of view of constitutional theory or of another phase of physical metallurgy. One trend in engineering is toward higher temperature requirements of metal. Consequently, we see considerable development of oxidation-resisting coatings, and alloy combinations themselves resistant under severe high temperature corroding conditions. Interest is naturally directed collaterally to the change in mechanical properties with the temperature. It cannot be said that physical metallurgy has played a great part as a creative influence in this field. Discoveries of especially useful alloy combinations have been made mainly by persistent experimentation. Improvements in metallurgical equipment and technique have contributed to this end. Thus, we have the developments in tungsten metallurgy, giving us, apart from the primary uses of the metal, valuable preparative and treating equipment in the form of tungsten and molybdenum furnaces. The Northrup high-frequency induction furnace offers ready means of attaining almost any temperature under non-contaminating conditions and accordingly has been much used in the preparation of

very refractory alloys and special products, such as carbon-free ferrous alloys.

Recent symposia by the American Society of Mechanical Engineers and the American Society for Testing Materials attest the widespread interest in this general field, and the literature emanating from these symposia, chiefly in the form of tabulations and comparations of the many alloys proposed for high-temperature service, will form a useful nucleus for critical studies in this field.

When the ordinary mechanical properties, ductility for example, are examined in their relationship to extensive temperature change, irregularities or discontinuities are commonly observed, and the theoretical interpretation of curves expressing such relationships must proceed hand in hand with the development of the newer constitutional theory. In order to control the property of rigidity through a temperature range, more careful study must be made of all slip-resisting agencies. Searching structural examination of promising alloys already developed by crude experimentation may supply new viewpoints, which can be further exploited under scientific guidance. Points of similarity between the mechanical behavior of metals which do not freely strain harden, observed at ordinary temperatures, and metals which do strain harden, observed at elevated temperatures, indicate that any fundamental means of increasing the rigidity of the former by experimentation at ordinary temperature might apply to the latter in the range of elevated temperature. It is, however, notoriously difficult to stiffen these soft metals much without a damaging loss of ductility.

Progress in any branch of technology scarcely waits for elaborate and complete theoretical guidance, but inevitably takes place whatever the methods at hand. Doubtless our future alloy developments will, like the past, rest largely upon widespread chance experimentation, but it can hardly be doubted that the foundation theories of physical metallurgy are reaching a point of practical utility and general appreciation which will influence the generality of creative effort as never before in the history of alloy technology.

MILITARY AIRCRAFT AND THEIR USE IN WARFARE.*

BY

MAJOR GENERAL MASON M. PATRICK.

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IN ALL of the conflicts in which contending forces have struggled for victory, up to the beginning of the World War, footmen and horsemen constituted the armies; and moving on the surface of the earth, they met each other in battle, and the issues thus decided often changed the political map of the world. The triremes of the Greeks and Romans, the sailing vessels that followed them, the dreadnaughts and superdreadnaughts of modern navies, all these surface craft have been used by man when he conducted his wars on the high seas. In all these wars, whether by land or sea, the fundamental strategic principles remain unchanged from earliest time and nothing can alter them. Man has, however, taken advantage of all of the discoveries in science and in the arts to increase his offensive and defensive power; has originated, invented new methods and new weapons of warfare. One of the most notable developments during the World War was the use of aircraft in battle.

Aircraft used in warfare are of two kinds, those technically known as lighter-than-air craft which are enabled to ascend by the lifting power of gases, themselves lighter-than-air, which are contained within them. Balloons and dirigibles, also called airships, are examples.

Second, heavier-than-air craft, generally known as airplanes. When moved rapidly through the air, the vertical component of the pressure of the air sustains them in their flight.

Of the lighter-than-air craft, the military forces use observation balloons, sausage or kidney shaped to prevent their rotation, anchored to the ground, and in the basket depending from them, one or more observers to do their work. This consists of noting within their range of vision enemy movements or other activities and the effect of our own artillery fire. As the observers can

* Address delivered Wednesday, September 17, 1924, on the occasion of the observance of the centenary of the founding of The Franklin Institute.

communicate the result of their observations by radio or telephone, they can regulate the fire of batteries and give timely notice of anything which they discover. They are fixed in position, although, of course, they can be moved from one position to another, the height they can ascend is rather limited and therefore the area they can keep under observation is restricted. Furthermore, during the World War they were very vulnerable and easily shot down by attacking airplanes, using incendiary bullets which set on fire the hydrogen gas used to give them buoyancy.

Fortunately, we ourselves, alone of all the world, can make use of helium gas, found here in sufficient quantities as a component of many natural gases to warrant its extraction. This gas has over 90 per cent. of the lifting power of hydrogen and is non-inflammable. It is of great value, not alone from a military point of view, but on account of the possibility of its use in commercial aircraft. Strenuous efforts are being made to conserve an adequate supply of it, but as yet nothing has been done, and this rare gas is being wasted at the rate of 500,000,000 cubic feet per year and the supply is not unlimited.

Dirigibles or airships are of three classes, rigid, semi-rigid and non-rigid, according to their construction. The rigid have a framework of light metal which keeps in shape the envelope covering over it and within the framework are gas-tight containers for the lifting gas. Semi-rigid dirigibles have a metal keel and the pressure of the gas keeps the envelope in shape. Non-rigid are merely gas bags. All types are provided with suspended cars for the crew and are driven through the air by one or more motors. These ships vary in size from the smallest used for training and having a capacity of about 100,000 cubic feet to the *Shenandoah* of the Navy and the new ZR-3 about to come from Germany of some 2,600,000 cubic feet capacity. England plans to build an airship twice as large as the *Shenandoah*, and indeed there seems to be no limit, except cost, to the size they may attain.

Military dirigibles can rise to considerable heights. Right here, it might be well to remind you of the formula which gives the distance of the visible horizon. D miles $= 1\frac{1}{2}\sqrt{H}$ feet. In other words, when a mile high the visible horizon is approximately 100 miles distant and the area abounded by the circle of the horizon is about 30,000 square miles. Dirigibles may readily reach the height of five miles. Thanks also to helium gas, our

dirigibles are reasonably safe from hostile attack. Their ability to hover over any point gives them further value from a military aspect. Their lifting power, or the useful load they can carry, is relatively great. For example, the Navy's *Shenandoah's* useful load is thirty tons, so that it can carry large quantities of supplies or a great weight of bombs. The radius of action is also large, thousands of miles, so that they may be used for long-distance reconnaissance or to transport supplies or troops to distant points, or for bombing operations against distant objectives.

One notable use contemplated for them is carrying airplanes, and our experiments have gone far enough to make it certain that airplanes can attach themselves to an airship while both are in flight and can take off from the airships at any time.

Helium gas is quite costly, and it is essential that as little as possible be wasted. As the airship's motors consume their fuel, normally this would lighten the load carried. Then when necessary to land, we should have to release or to valve some of the gas to lessen the buoyancy. Fortunately, the Air Service developed and the Navy is using what we call a water recovery apparatus by which the water vapor contained in the exhaust gases from the motors is condensed and for every pound of gasoline consumed, we get about a pound of water, so that the weight of the airship is kept practically constant and the necessity of valving gas when landing is avoided.

Heavier-than-air craft or airplanes were used for military purposes for the first time during the World War. The far-reaching importance of the proof given by the Wright brothers that man could actually fly was scarcely recognized in their own country. Foreign governments, or at least their military men, seemed more alive to this, and at the outbreak of the war, England, France and Germany had each a few crude planes, though Germany placed her main reliance in her Zeppelins and, as we know now, had made her plans in advance for destroying Paris and London by bombs dropped from them. Airplanes were at first used solely for observation purposes, visual reconnaissance. The airmen themselves knew little of what they could or should do and the commanders of the ground troops knew less. The value of the information thus secured was, however, soon grasped. From time immemorial, army commanders have sought or desired vantage points from which they could see what was taking place during the

progress of a campaign or a battle. It was Wellington who said he would give half his army if he could know what was going on beyond yonder hill. The advantage gained by the height airplanes can attain has already been pointed out. Moving rapidly, they can cover great areas and again they can convey quickly to the proper place the result of their observations.

It was soon seen that while important to obtain information, it was most desirable to prevent it from reaching the enemy. Then came the era of air fighting with planes specially designed and built for this purpose.

As is usual under war's stress, developments came rapidly. The speed and carrying capacity of planes increased greatly. The uses to be made of them multiplied. And so, to-day, profiting by the experience gained in the war and by our subsequent study, we have airplanes of four different types, each intended for a specific use, although it may be well to say that under exceptional conditions planes of any type might be employed to do the work normally required of another type or of other types.

The first, the so-called pursuit or combat planes, single seaters, armed with two machine guns, their prime mission to seek out and to destroy enemy airplanes. They are fast, very manœuverable and our present service type can make about 165 miles per hour with its military load. We shall soon have such planes capable of making 185 to 200 miles per hour. They must be able to climb 10,000 feet in a little less than ten minutes and capable of operation at altitudes of from twenty-five to thirty thousand feet. It is essential to obtain command of the air in order that our own planes may be reasonably safe from enemy attack. This is the function of the pursuit planes and pilots. They are instructed and trained to know that while they are themselves making it safer for our slower planes to operate, the protection thus afforded is incidental, it is given by the execution of their own prime mission, the seeking out and destroying enemy planes. At almost the end of the World War, fighting planes operated independently and single combat was joined by individual planes. This practice has been supplemented by effective aerial formations, involving the fundamental, tactical principle of support and reserve. This in order to prevent the dissipation of air forces and in order to bring the greatest possible concentration of attacking power on the enemy formations, striking him on converging lines. Normally,

these pursuit formations fly at three different elevations, the highest being the reserve, the next highest the support.

Our observation planes are two-seaters, carrying a pilot and an observer, who supplements his visual reconnaissance by photography, the planes being equipped with specially designed aerial cameras. Our service type plane is still the war-time designed DH, named after its English designer, which can make about 120 miles per hour. This is known to be too slow and, before long, it will be replaced by a plane of American design, faster by some forty miles per hour. Observation aviation is organized, trained and equipped to report upon the disposition and activity of both enemy and friendly ground and naval forces, to observe and to regulate artillery fire. It is thus in reality the eyes of the Army. The observation airplane flies above, observes and notes all enemy movements, likewise troop centres, supply depots, rail heads, bridges and accurately reports to the army commander what the observer has learned. In future wars, army and subordinate commanders will no doubt use aircraft to obtain first-hand information of what is taking place.

The aerial photographs taken by observation planes show clearly what the eye of the camera sees and experience proves that it is practically impossible to hide any military activity from this searching eye and that even camouflage methods do not avail. While it is possible by camouflage to deceive the eye of even a skilful observer, the camera generally detects these efforts at concealment and frequently indicates sufficiently clearly what they have attempted to cover. These observation airplanes carry signal apparatus, enabling them to communicate with ground troops. This makes it easy to adjust artillery fire by reporting to battery commanders their errors, deflections, overs and shorts and the effect of their fire upon their target. The long range of modern artillery and the conditions which prevail on the battlefield make it practically impossible for any other form of observation to be used effectively. Furthermore, as the airplane can fly directly over a ground target, by visual reconnaissance and by photography, they can spot with great accuracy the places where artillery projectiles strike.

Our service bombing planes can carry one 2000-pound bomb or a number of smaller ones of this same aggregate weight. Of course, airplane bombs, not subject to shock as are shells from

cannon, carry a larger percentage of explosives. For example, our 2000-pound bomb carries about 1000 pounds TNT while the projectile of a 16" gun, weighing about the same, carries only fifty-five pounds of explosive. Cannon shot are expected to burst after penetration, airplane bombs on contact, so that the relative effect is not directly shown by the respective amounts of explosive.

These bombing planes are used to attack all sorts of enemy objectives, both on land and on sea. Their radius of action is comparatively great and they can be sent on distant missions far beyond the actual front lines of the army.

The fourth type, attack planes, are intended primarily for use against ground troops. They carry a number of machine guns, small bombs and, if so desired, cannon as well. When cannon are used, it is necessary to provide for, or to eliminate, the recoil which would bring too great a strain on the airplane; and this is done cleverly in the Davis gun, a barrel open at both ends, the powder charge in the middle, on one side of it the projectile, on the other an equal weight of small shot.

It must be quite evident that airplanes of the different types and for the different uses must be differently designed. What we call the characteristics of aircraft are speed, rate of climb, ceiling (the height they can climb), the manœuverability, the weight-carrying capacity and endurance, the length of time they can stay in the air, which obviously is determined by the fuel capacity.

It is possible in designing an airplane to stress or emphasize any one of these characteristics, but it will be understood that this is always at the expense of all of the others. The power which a given motor can develop is fixed. Speaking broadly, we can use this power as we will. If it is great speed we want, first of all we must sacrifice weight-carrying ability and the converse is likewise true.

The result is that the design of practically every military airplane is a compromise. There is a minimum load which must be carried, manœuverability, too, is essentially important in planes which must attack or which must defend themselves from attack. The average pursuit pilot who knows that in combat his own life is at stake will prefer a plane easily manœuvred even though this means the sacrifice of speed.

We are continually developing and improving our airplanes and the advance in design is already very great. Just sixteen years

ago when we bought our first army airplane from the Wrights, it was required to make forty miles per hour and to carry, besides its fuel, the equivalent of 350 pounds. It was doubted whether these severe requirements could be fulfilled. To-day, a speed of over 250 miles per hour has been made and the Army's Barling Bomber weighs 26,000 pounds, can carry a load of 15,000 pounds and can fly with it at the rate of eighty miles per hour.

In dealing with the missions of military aircraft, so far I have dwelt almost entirely upon their use against land objectives, enemy troops, enemy establishments or other land activities. It must not be forgotten that they can also be used against water craft and most effectively.

During the World War, little use was made of aircraft against hostile shipping, against transports or against naval craft, except against submarines. Notable experiments carried out by our own Air Service in 1921 and again in 1923 proved conclusively that bombs delivered from aircraft could put out of action or sink any naval craft which up to this time has been designed or built. Phosphorous bombs, striking on a vessel, make it impossible for anything on its decks to live. Direct hits by bombs carrying high explosives do great damage, but it was also shown that it was not necessary to make a direct hit in order to sink any naval vessel. Bombs exploded under water and near a vessel were sufficient to send it to the bottom, or even when it did not accomplish this, the blow dealt might be so staggering that the first effect would be to put the vessel out of action. While the aircraft in these experiments operated freely and without being themselves exposed to attack, we of the Air Service are satisfied that no fire can be delivered from a vessel's deck sufficient to make her safe from an attacking air force.

Furthermore, it may be pointed out that had the vessels when attacked been in commission, steam in their boilers, their communication and lighting systems in operation, it is quite conceivable that the explosion of an aircraft bomb, even though it did not inflict vital damage, might so injure the machinery and other parts of the operating mechanism of the ship that it would be no longer a factor in a naval engagement.

These facts make evident the important rôle aircraft will play in coast defence. Scouting far out to sea, airplanes or dirigibles discovering an enemy fleet approaching the coast, give timely

warning, and an air force of adequate size can be concentrated where needed to oppose the impending attack. Assuming such a fleet is discovered, say 200 miles off the coast, at the normal average speed of naval craft, there would be a period of some eight or ten hours before it would be close enough to do any damage. This gives ample time to move an air force long distances and this emphasizes a further advantage, great mobility, which such an air force possesses. Recently as a test, the Army Air Service moved a bombing group from near the mouth of the Chesapeake Bay to Bangor, Maine, in about eight hours, and within half an hour after it reached its destination, it was in readiness to operate. Such an air force located at proper stations in the interior of the country or along the seaboard could be flown to any threatened point promptly and effectively. Considering the long extent of our own coast line, it is readily apparent that this is a fact of extreme importance. Remember also, that the Air Service has flown entirely across the continent without stopping in only about twenty-six hours, and with a faster plane, making some four or five stops, has traversed the distance between New York and San Francisco in the light of a single day.

The Round-the-World Flight planes passed over the city of Philadelphia just eight days ago, their long journey nearly done. This flight, about completed, has taught a number of worthwhile lessons. From a military standpoint, this flight has shown that no distances or no difficulties are great enough to make any country immune from attack by aircraft. On the other hand, and to stray from the subject a moment, this same flight has shown how communication between the nations of the world can be improved, how parts of the earth otherwise almost inaccessible or without sufficient means of transportation and communication, can be brought nearer and thus enable men to know each other better.

As to the possibilities of the use of aircraft in future wars, it may be worth while to quote some of the opinions of others. General Duval, who was Chief of the French Air Service during the war while I was in charge of the Air Service of our own Army, says:

"The primary objective of the air force will be the enemy armies and especially those parts of them in the areas beyond the fighting line in which it will carry disorder and terror. It will

fight in the air for air supremacy and the liberty of action which follows. Its power will grow with the number and development of its airplanes which will be more heavily armed, will be speedier, and will have greater radii of action. The battle will no longer be confined to the zone occupied by the troops. In fact, the object of the attack may no longer be the opposing army, as this may be neglected and the desired results may be obtained by a campaign of terror carried on against the enemy country rather than against its armed forces. The decision will be reached in the air and the victor will dictate peace on the ground."

Colonel Fuller of the English Army pictures thus an attack on ground troops:

"We see columns of foot and horse wending their way toward their battle areas, whole divisions twelve miles long toiling along dusty roads. Then we see in the distance tiny specks on the horizon, they grow bigger, there is a droning of engines—twenty low-flying armored battle planes top the rise in front, and, before the wretched infantry have time to unstrap their limbered vehicles and mount their machine guns, there is a rattle of musketry and in ten minutes the whole column is traversed from front to rear, 250,000 bullets have been pumped into it and the very dust of the highway is churned into a porridge of blood. Such warfare as this is truly horrible, because it is so one-sided. To shoot down infantry in this manner is mere massacre."

In 1921, Marshal Foch said:

"The potentialities of aircraft attack on a large scale are almost incalculable, but it is clear that such attack, owing to its crushing moral effect on a nation, may impress public opinion to the point of disarming the Government and thus become decisive."

The late Premier of England, Bonar Law, said:

"The whole centre of gravity has changed in the matter of national defence from the sea to the air."

In a speech that Lord Birkenhead delivered on May 9, 1923, he said in part:

"In every field of military preparation, it has been the habit of this country inveterate, incorrigible, to refuse always to anticipate those crises which experience and the study of history have, none the less, taught us over long periods to be inevitably recurrent.

"In the last few years an immense and incalculable permanent change has taken place in conditions which is vital and funda-

mental, and upon which the security and very existence of this country depend. It has become clear, beyond a doubt, that it is no longer possible to estimate the security of this country in military or naval terms, but that there has arrived a third term, namely, that of an aerial security.

"This problem of air defence is, in the years that lie in front of us, as vital a question as the strength of the navy was to those who preceded us in Parliament in the last thirty years."

The English Admiral, Mark Kerr, has written:

"War is an affair of communications. No admiral or general would attempt any move unless his communications were assured.

"There is only one arm which can strike at once a real blow at each one and all of the above links in the chain of the enemy's communications. Each side will then endeavor to be the first to attack by air and unless we are ready to do this, our people will feel war as they have never felt it in all their history. Poison gas, poison acids, high explosives, and incendiary bombs will be raining from the air wherever there is a factory or arsenal to be destroyed.

"Above all, remember that, where as in former times it took days, weeks or months before any heavy blow would be dealt after the commencement of war, now, with the new power of the air, a fatal blow may be delivered from the air which may practically finish the war within a few hours from the time when the enemy made up his mind to attack."

General P. R. C. Groves wrote the *London Times:*

"War, if it should come again, will begin with an aerial phase which may in itself be decisive. Those not in close touch with the enormous progress in aviation since the Armistice are inclined to underestimate air power and to judge it by the pin-prick air raids of the past. The total weight in bombs dropped upon London in the course of the World War was about twelve tons. To-day several thousands of tons could be dropped on the capitol in the course of twenty-four hours."

The well-known war correspondent, Frederick Palmer, wrote:

"The French reasoned: 'The thrift, skill, and industry of our people will look after our economic recovery. Meanwhile, we must have a card to play against British interference with our plans for security.'

"The card was on the table the day of the French entry into the Rhur in the form of overwhelming French air squadrons which could fly over any number of British battleships in the Channel and drop a shower of bombs upon the British coast and vital points inland. No longer has France to build a navy as a check to British power while she maintains an immense army against Germany. She has a force in the air which she can throw in either direction. This means a revolutionary change in Britain's tactical relation to the continent, which has existed since Cæsar saw that, if he were to include the Angles in his triumph, he must have boats."

At any rate, there are two great lessons which were taught by the World War, the importance of which has been only emphasized by our subsequent study. First of all, an army without an air force contending against one so equipped is practically helpless. I have called the Air Service the eyes of the army. Without it, an army is blind and could no more fight effectively than could a blind man opposed to one who was able to see to parry every blow aimed at him, to plant at will upon his helpless antagonist the blows which he himself deals. The second and even greater lesson is that there is no effective defence against aerial attack except an air force of our own. Much is made by some of the possibilities of anti-aircraft fire and while such a method of defence must still be employed and may, under certain circumstances, be reasonably efficacious, it could not be relied upon to prevent absolutely an attack by aircraft. Our own experience during the war bears this out. Of all of our losses of aircraft and airmen, only about 1 per cent. were attributable to anti-aircraft fire. General Groves of the English Army states that in all the German raids over England during the World War not more than six airplanes were brought down by anti-aircraft guns.

With particular reference to attack upon naval vessels, the Air Service with the assistance of the Chemical Warfare Service has developed material for placing smoke screens which will absolutely blanket a target and prevent those on shipboard from noting the approach of attacking planes. Fast-flying airplanes can actually surround a target with such a screen and, of course, similar smoke screens can be used when attacking land objectives.

Again, even though the means just described might not be as efficacious as expected during daylight hours, surface vessels still have to run the gauntlet of night attack, the airplanes carrying parachute flares which, when lighted, illumine the surface of the sea for miles while the planes flying above this source of light are absolutely invisible. Nor will the searchlights on shipboard greatly aid in detecting oncoming or attacking planes at night. The beams of these lights are easily avoided and the lights themselves will but add to the ease with which the airman discovers his victim.

These airmen, while seeking the target, or on whatever mission they are engaged, can communicate with each other by wireless telephones, giving and receiving information, or by this same means, the commander of an air force can readily direct its movements. This means of communication is being constantly improved. To-day, planes in the air can talk to each other up to a distance apart of about ten miles. The airmen can talk to a ground station some 100 miles distant and the ground station can communicate with the plane when it is 100 miles away.

The Air Service has developed what, for want of a better name, we may call the automatic pilot. It is possible to-day to direct a plane towards a given objective, toward which it will fly by itself at a pre-determined height and at a prefixed time it can release the bombs which it may carry. Planes similarly equipped may also be guided by radio and it is not at all improbable that in the near future a number of such planes, none of them containing a human being, may be directed and guided by a man in another plane several miles away, steering them over their objective and causing them to drop their bombs at his will. This is another answer to those who claim that anti-aircraft may make airplane attack ineffective; another reason why we claim that the only adequate defence against air attack is opposing aircraft.

History could have taught us the same lesson even without actual experience in warfare. Whenever man has developed a new weapon superior to those previously in use, he has held his opponents helpless unless they too armed themselves with similar new engines of destruction. In earliest times men fought, and man has been fighting since he appeared on earth, with the weapons nature gave him, his hands, feet and teeth. Then some

one of these ancestors of ours grabbed a club, and had not his antagonist done the same, this club wielder would have been king of his little world. Later, bows and arrows came into use and the archers dominated the field of battle until their opponents fashioned for themselves the same weapons. The mail-clad warrior was invisible when opposed by those not similarly caparisoned, but he, too, was vanquished by muskets and cannon and the sun of the day of chivalry went down in the clouds of powder smoke.

Therefore, it is repeated that the only defence against air attack is an air force of our own. So thoroughly is this grasped by military men that few to-day dispute it, and this is true not alone of land armies, but of navies as well. No fleet in the future will dare put to sea unless it has with it its air component. Airplanes will be used by the Navy for scouting, for carrying bombs and torpedoes. They must likewise have their combat planes or else the slower observation and bombing planes will be destroyed by enemy aircraft.

The fundamental military or strategic principles upon which war is fought are unalterable. For example, the definition of strategy attributed to General Forrest: "Getting there first with the most men," still holds good, but the coming into being of aircraft and their use in warfare does change profoundly our geographical isolation and materially reduces the time in which we might prepare for our defence after war is declared. The fact that we must have aircraft for military use will have a bearing upon the economic phases of our national defence problem and once more and for the sake of emphasis, it is again repeated, that the only effective defence against an air attack is an air force of our own.

These facts should be impressed upon our minds and the necessary action should be taken, in order that we may have the feeling of national security, the consciousness of our own power, of our ability to defend ourselves, which will strengthen our arm when we must raise it in the cause of right.

Furthermore, it has been pointed out that aircraft make it much more easy for men to know each other and with such knowledge comes understanding so that these, the most potent military machines ever devised by man, may likewise be agents for promoting the peace of the world.

A Method of Distinguishing Natural Pearls from Those Artificially Grown. A. DAUVILLIER. (*Comptes Rendus*, Oct. 27, 1924.)—Direct examination with X-rays makes it possible to detect the pearl in the unopened oyster, but the observer cannot tell a good natural pearl from one grown by the Japanese procedure around a core of mother-of-pearl. The two pearls look alike, have the same chemical nature and possess almost the same density. The only sure way of distinguishing one from the other is to cut the pearl open and examine the structure.

The author has tried to solve the problem by applying the method that has yielded such valuable results in the case of crystals. Monochromatic X-rays, the K-rays of rhodium or of silver, were sent through the pearls and the resultant figures were examined after several hours of exposure of the photographic plate. Good natural pearls showed a series of regular rings, sometimes sharply marked, while mother-of-pearl presented in every case Laue figures more or less regular and made up of patches more diffuse than in the case of crystals. These have a hexagonal symmetry when the rays are perpendicular to the cleavage planes, but are more numerous when the rays are parallel to the planes. Artificially grown pearls show a combination of the two figures just described, and can thus be distinguished from the natural gems. G. F. S.

Measurements Concerning the Electrical Resistance of Indium in the Temperature Field of Liquid Helium. W. TUYN and H. KAMERLINGH ONNES. (Communications, Phys. Lab., Univ. of Leyden, No. 167.)—"For the further detection of supra-conducting metals it seems desirable to investigate the behavior of those elements which take a place near already known supra-conductors in the periodic system. Indium—above thallium and by the side of tin—seemed a suitable metal." Accordingly from chemically pure indium four coils of wire were prepared with resistances ranging from 3.7 to 4.7 ohms. These were placed in a helium bath, the temperature of which was determined by measuring its vapor pressure. At a temperature close to 3.4° abs. all the coils possessed no resistance that could be detected. For the three best coils the resistance at 3.42° abs. was about 1/30 of the resistance at ordinary temperature. G. F. S.

The Constant σ of the Stefan-Boltzmann Law. A. KUSSMANN. (*Zeit. f. Physik*, Vol. 25, No. 1.)—The radiation in watts from 1 sq. cm. of a black body in a vacuum is calculated by multiplying the constant σ by the fourth power of the temperature of the radiating area. Since 1898 twenty determinations have been published with results ranging from 5.3 to 6.5×10^{-12} watt/cm.^{2}. In this paper a careful redetermination is made and the final value presented is 5.79. This lies between the result of Coblentz and Emerson in Washington, 5.72, and that of Gerlach in 1912, 5.80. G. F. S.

THE FIELD OF RESEARCH IN INDUSTRIAL INSTITUTIONS.*

BY

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IN THE presence of such distinguished scientists, I hesitate to speak on any phase of scientific work, as I realize that I have no qualification beyond a deep love of science, and the good fortune of a life-long association with scientific men.

On this occasion, our thoughts naturally turn to Benjamin Franklin, that remarkable man, sage, philosopher, statesman and scientist, whose name this institution has borne for a hundred years—an institution which, during the century of its existence, has performed a service which has shed lustre upon the illustrious name which it bears.

Franklin's research into the nature of lightning, and its identification as electricity, by drawing the lightning from the clouds in the famous kite experiment, was not only remarkable for the time in which he lived, but would, I imagine, rank with the best methods of modern scientific research, as a model of simplicity of apparatus, and equally convincing in its solution of the problem.

The lightning rod which he invented for the protection of buildings and similar structures was, perhaps, the first illustration of the practical value of scientific research, and may, without too great a stretch of imagination, be considered as marking the beginning of our electrical industry.

Franklin was elected a member of the Royal Society of London without his solicitation, and was excused from payment of dues. He was, so far as I am aware, the first American to receive this great honor. While the Royal Society honored itself in thus honoring Franklin, he himself greatly appreciated the gift, and especially the manner of its bestowal.

We all think of the Royal Society as the Mother of Research, and the vision of Davy, Faraday and Dewar, to speak only of a few of those who have gone, immediately arises in our minds.

* Address delivered Thursday, September 18, 1924, on the occasion of the observance of the centenary of the founding of The Franklin Institute.

If we wish to be instructed as to the best methods of conducting research or of recording its results for the benefit of the world, we turn instinctively to the inspiring records of Faraday's work.

Davy's discovery of the electric arc and Faraday's discovery of electromagnetic induction were the foundation upon which, years afterward, was built our electrical industry.

Dewar's scientific research into low temperatures led to his invention of the "thermos bottle," which has become one of the conveniences and necessities of our modern life, and the basis of a large industry.

The activities of industrial institutions range over a field so vast that, in order to free myself from an embarrassment of riches, I propose to confine my remarks principally to the electrical industry. Even such a restriction will not prove much of a limitation, as the electrical industry touches all other industries, and, in fact, every phase of human activity—agriculture, mining, forestry, water power, steam power, transportation, communication, in fact, nearly everything indoors and out-of-doors.

The difficulty may be due to the all-embracing nature of electricity, which, we are taught, is the substance or entity, atomic in nature, out of which all matter is produced.

Nowadays when we speak of research, we usually have in mind scientific research. Industrial research may not only partake of the nature of pure scientific research, but, reaching into a broader field, interests itself in the applications of science to lighting, power, chemistry, physics, medicine, mechanics, education and sociology, every phase, in fact, of human endeavor.

At the same time, it is not easy to satisfactorily define "industrial research," as the same event or process may be looked upon as research by one, development by another, and mere improvement by still another.

We experience a similar difficulty in attempting to define "field of research," as industry itself is constantly changing, forever expanding, the field of research of to-day becoming the field of technical development on the morrow, and that of commercial application the day after to-morrow.

Mr. C. F. Kettering, President of the General Motors Research Corporation, has attempted to define the "field of research" by a process of exclusion, saying in effect: "If you have no problems in your business, if you are perfectly satisfied with

your product, your processes and your costs in all respects, if you have no troubles from competition or other sources of worry, and are sure you are not going to have any for ten years to come, then you *may* not need research." It is obvious that Kettering used the word "may" quite advisedly.

What business is free from problems and troubles and worry, even for a day?

There was a time, about 1911, when one might have thought that the incandescent lamp business in America would fairly meet Kettering's specifications. We were very happy and proud of our latest tungsten lamp, with its filament made of tungsten wire, an original and remarkable product of our research laboratory. Why should we worry, why not rest in "comfortable complacency" after our strenuous labors? But our restless researchers decided otherwise. Even then, they were discovering new laws governing the conduction of heat from wires to surrounding gases, a research which resulted eventually in a still more efficient product, the gas-filled lamp.

I remember, about the year 1900, having a conversation with several eminent scientists, in which the discussion turned to the peculiar position of carbon in the electrical art. Carbon had been found to be the only substance which could be used for the electrode in the arc lamp, for the filament in the incandescent lamp, and for a brush for the commutation of direct current dynamos. It was agreed that probably nothing else would ever be found to take the place of carbon for such purposes. It seemed as though carbon had been created especially for use in the electrical art, along with copper for conductors and iron for magnetic purposes. We felt secure in our opinion, as all the years of the industry had disclosed no substitute.

In less than ten years, a great revolution took place, and tungsten displaced carbon in the incandescent lamp, and magnetic electrodes were found to give very superior results in certain arc lamps.

I recently re-read an address given by that master of industrial research, Prof. Elihu Thomson, twenty-five years ago before the American Association for the Advancement of Science, on "The Field of Experimental Research."[1] While the entire address is worth reading, even at the present time, I will only quote a few paragraphs that seem especially pertinent.

Speaking of experimental research, which is one phase of industrial research, he says: "Whatever may be our ideas of fundamental entities, as expressed in various theories, whether, as an example, we regard the ether as like an infinitely mobile fluid, or as an incompressible solid, or as a jelly, or whether we are inclined to think that, being an electromagnetic medium, it may be without mechanical properties, which properties depend in some way upon the electromagnetic nature of the ether, we cannot reach sure ground without the experimental test."

He emphasized the interdependence of scientific and industrial research, saying of the industrial researcher, "He must learn to render available to science the resources of the larger workshops and industrial establishments. The application of physical principles upon a large scale in such works has frequently, in recent years, resulted in great gains to science itself. The resources of the physical laboratory are often relatively small and meagre compared with those of the factory. Experimental work in certain lines is now frequently carried on upon a scale so great and under such varied conditions as would be almost impossible outside of a large works. In no field has this been more true than in that of electricity during the past few years."

Professor Thomson gave this address in *1899*. Many of those in my audience will easily remember the remarkable development in electrochemistry which took place during the decade which was then just ending, starting with the development by Hall of the process of making aluminum in 1890, followed by the discovery of carborundum by Acheson in 1891, of calcium carbide by Wilson in 1892, all the result of scientific research, and each in turn the foundation of extensive new industries, the creators of new, untold wealth.

Then in 1894 came the discovery of the mysterious X-rays by Röntgen. We had scarcely time to become accustomed to this great marvel when Becquerel in 1896 announced his discovery of somewhat similar rays continuously shooting out from the metal uranium. Two years afterward the Curies made the great discovery of radium, and its even more powerful radiations.

Sir Ernest Rutherford says, "Even the most imaginative of our scientific men could never have dreamed at that time of the extension of our knowledge of the structure of matter that was to develop from these two fundamental discoveries."

This same decade witnessed Marconi's feat of sending a message by wireless across the English Channel, the technical and commercial development of the electric trolley, the general introduction of alternating current generation and transmission of electric energy, the harnessing of Niagara, and countless other achievements too numerous to even mention.

If an examination were made of the history of these developments, it would be found that they all owed their origins to scientific research.

Remarkable as is the above incomplete list of notable advances in a brief period of less than ten years, it will be remembered that almost any decade during the last fifty years may be credited with an equally wonderful record in the electrical field alone, beginning with the telephone of Bell and the incandescent lamp of Edison in the seventies, to the latest radiant children, wireless telegraphy and telephony, and radio broadcasting.

When it is realized that all this marvellous progress is founded upon the "experimental method," or, as we now call it, the "research method" of interrogating and attacking nature and forcing her to yield her secrets for our benefit, is it any wonder that men have been impressed by the value of the scientific method, and that research work and research laboratories have increased so rapidly during the past few decades?

"Research" in the dictionary is defined as a "systematic investigation of some phenomenon or series of phenomena by the experimental method to discover facts or to coördinate them as laws." L. A. Hawkins qualifies this definition by the statement that he would "limit the facts sought by research to generic facts," and points out that "it may be desirable to know the open-circuit voltage of a certain battery to another decimal point, but to obtain that fact by refinements of measurement is not research. It does not give us the kind of new knowledge or new principle which makes possible the development of new devices. Again, an industry may need a developmental laboratory to improve its apparatus, to develop new devices, and to find new applications for its products, yet none of it may be research. All these things have to do with specific things; they represent applied science or engineering. Research, on the contrary, has to do with generic facts or principles. It need have no connection with industry. It is fundamentally the same whether conducted at a university or in

an industrial laboratory. It is essentially pure science until engineering development steps in to apply it." [2]

It is evident that in the above comments, Hawkins is speaking of research in pure science, and that for industrial research we require a broader definition. Perhaps we may define "industrial research" as a systematic search for information useful to the industry. It is obvious that the more we learn about our business, the better; that we may then direct its expansion more wisely, increase its usefulness, and insure its relative permanence.

To quote Rutherford again, he says, "In this age no one can draw any sharp line of distinction between the importance of so-called pure and applied research. Both are equally essential to progress."

In this country, up to comparatively recent years, in industrial circles, the word "laboratory" was commonly used to designate chemical laboratories. These were usually small, and principally engaged in testing raw materials, and were not in any sense research laboratories.

There were in existence, in addition to such chemical laboratories, testing departments for the testing of materials purchased, and experimental departments generally confined to one or two rooms, called "model rooms" or "experimental rooms," in which new inventions and improvements relating to the industry were developed.

In the electrical industry the realization of the value of scientific methods was perhaps greater than in the older, well-established industries. There was a closer connection between the universities and technical schools and the electrical industry in its earliest days. The newness, combined with the highly technical nature of the electrical business, led those in charge of the engineering work to draw upon the colleges and universities for graduates trained in such scientific and technical knowledge as then existed. These men were set at work in the engineering and testing departments, and those of special aptitude were given opportunities in experimental departments to assist in the research and development necessary to a new industry.

As is well known, the field covered by the electrical manufacturing industry includes nearly every article of an electrical nature —dynamos, motors, transformers, regulators, meters, measuring instruments, switchboards, lightning arresters, insulators, arc

lamps, incandescent lamps, electrical heating devices, X-ray tubes, radio apparatus, steam turbines, electric locomotives, and so on, almost indefinitely.

Of course, such a variety of articles calls for the use of about every known material, all metals, woods, paper, oils, coal and tar, glass and porcelain; the utilization of about every known physical, chemical and mechanical process, not to mention other human activities. Naturally, every article, all the materials and processes are subject to improvement, and therefore theoretically form a legitimate subject for industrial research.

In order to provide for the development of its apparatus and improvement of its processes, it has been found desirable, especially in the larger industrial corporation, to establish numerous experimental or developmental sections or departments in many different localities. These have been established as needed over a long period of years. The field covered by such experimental departments is coextensive with the product.

Naturally, it has been impossible to confine all experimental work to definite physical locations or buildings. Much research work is of necessity carried out in the manufacturing departments, and especially in the so-called testing departments, the experimental work and the research work in such cases being under the charge or guidance of the interested, responsible engineer. While a large part of such work may be more properly called development, as distinguished from research, there is an astonishing amount of truly scientific research constantly going on in the electrical industry, research that scientists of note and reputation regard as satisfying the most exacting definition of scientific research.

The oldest experimental and research unit of the General Electric Company is located in Lynn, Massachusetts, and is known as the Thomson Research Laboratory. This laboratory is the outgrowth of the Experimental Department which was started by Prof. Elihu Thomson over forty-two years ago. It has been the scene of many industrial researches, discoveries and inventions, mainly due to the genius and industry of its founder. Among the notable achievements may be mentioned electric welding, investigations of alternating current repulsion phenomena, flame arcs, constant current transformers, lightning arresters, integrating watt meters and others too numerous to detail.

At the present time, under the direction of Professor Thomson, a staff of about twenty is kept employed on research and development problems. On this staff are physicists and chemists, mechanical and electrical engineers. The field of research and development covers problems connected with steam turbines, turbine-compressors, superchargers for airplanes, special alloys, fused quartz and its application to insulators, lenses, optical reflectors and other purposes.

At Pittsfield is located the largest factory for the manufacture of transformers for power purposes, ranging up to 20,000 kw. In its laboratories and experimental department constant research has been carried on to improve the quality of the sheet steel used in transformers, and the oil used for insulating purposes.

Transformer oil, even of the very best quality, is subject to certain deterioration, called sludging, apparently as a result of oxidation. Research has been carried on for years to discover the exact cause and a remedy for the disease. Much promising work has been done with substances called anticatalysts. Tests of dielectric characteristics of a large number of oils and waxes are made, investigations of several years' duration are under way to determine the rate at which the mechanical strength of various types of insulation deteriorates when subjected to normal temperature of operation. This is an attempt to obtain accurate data in regard to deterioration of organic materials under operating conditions.

In the high potential laboratory at Pittsfield attempts have been made to reproduce artificially the conditions of a lightning stroke. Starting with 250,000-volt equipment some years ago, there is now in operation a 2,000,000-volt lightning generator, according to a design first suggested by Peek.

Early in the year 1923 lightning strokes of 2,000,000 volts to ground were for the first time in history artificially produced and controlled in this laboratory. These voltages are in excess of any hitherto produced, and are probably higher than those ordinarily produced on a transmission line by natural lightning.

Two million volts to ground lightning strokes can be obtained which increase at the rate of fifty million volts per second, while the power may be millions of kilowatts. The instantaneous value of the current may reach 10,000 amperes. Naturally, the results obtained are extraordinary. A discharge takes place with a very

loud noise. Large pieces of wood are split with ease; glass, porcelain, hard rubber are easily shattered; fine wires are vaporized and sheet metal exploded or punctured. Though the lightning strokes in some cases last only a millionth of a second, the eye can perceive the corona produced within this time.

Perhaps the most interesting theoretical result has been a more accurate estimate of the voltage of a real lightning bolt. Tests made by this 2,000,000-volt generator would indicate that the ordinary voltage of a severe lightning stroke to ground may be about one hundred million volts. If we assume that this corresponds to a discharge from a cloud about 1000 feet above the earth, it would be equivalent to about 100,000 volts per foot, which indicates a very fair continuation, even for these high voltages, of the needle gap curve, first published by Steinmetz, and afterward extended by Peek. Studies were made of the protective area of a lightning rod, and Franklin's theory that the lightning rod protects a definite area received experimental verification.

One may inquire how such industrial research could prove of value to the corporation which finances it. When originally started, it was justified on the ground that a better knowledge of such phenomena would extend the limits of long-distance electric power supply, and thus increase the demand for such electrical apparatus. It has already done this, and supplied data for improved design of such apparatus. It has also made possible important additions to our theoretical and practical knowledge of the dielectric strength of air and other materials.

The results of some of the research and experimental work of this laboratory are incorporated in a paper to be presented by Mr. F. W. Peek, Jr., at this meeting.

In 1901, the General Electric Company felt the necessity of adding to its already existing laboratories and experimental departments, a laboratory devoted to the definite purpose of undertaking research in pure science. Such a laboratory was started in Schenectady under the direction of Dr. Willis R. Whitney. The success of this laboratory in making important contributions to pure science, as well as in the creation of new products, is well known and does not need to be elaborated at this time.

Perhaps a concrete idea of the field for research which has already been found desirable may be obtained by considering subjects which have proved important enough to be placed upon a

fairly permanent list in the field of work covered by this laboratory, items which have been the subject for research for years past, and are likely to continue to be of interest for an indefinite period. A partial list is as follows:

Vacuum tubes,
X-ray tubes,
Electronics,
Incandescent lamps,
Kenetrons,
X-ray spectrum analysis,
Insulation,
Ionization,
Radio research,
Vibrations and strains,
Ferrous alloys,
Non-ferrous alloys.

One of the best examples of an industrial research laboratory, noted for the varied scope of its work, the revolutionary character of its output, and for the length of time which it has existed, is the famous laboratory of Thomas A. Edison. This was started in Menlo Park about 1875, and moved to Orange, New Jersey, about 1887, so that it has had a continuous history of nearly fifty years.

It was in Menlo Park that Edison conducted those remarkable researches and experiments which ended in giving the world not only the incandescent lamp, but a complete system of generation, distribution, regulation and measurement of electric light and power, upon which has been based the development of our central station electric industry.

From this laboratory also came the phonograph and moving picture. It might be claimed that the phonograph was an invention of a great genius, and not the result of research methods, as now understood. Yet even so, its development has required the application of the best industrial research methods.

Also, if Mr. Edison could be persuaded to write the story of the development of his nickel-battery, it would, I believe, disclose one of the most interesting chapters in the history of industrial research.

It is interesting to note that the vacuum bulb which Edison made a practical device in the incandescent lamp, is not limited in usefulness to the giving of light, but is the basis of a whole series

of useful electrical devices. It is used in the rectifier for changing alternating current into direct current, and the reverse; and the same vacuum tube, through the Edison effect, discovered by Edison in 1883 and developed by Fleming, Deforest, Langmuir and others, has become the basis of wireless telegraphy and telephony. This same Edison vacuum tube, in the hands of Langmuir, Coolidge and others, may be destined to revolutionize our present method of transmission of electricity. Therefore, the vacuum tube of Edison, although forty-six years old, is still one of the most interesting and promising of all the wonderful products of his genius.

Industrial research in the electrical business cannot be limited to the research laboratories, experimental departments and the various manufacturing establishments. It is necessary to make experimental investigations of phenomena on a larger scale than can be provided in any laboratory or even in any works, for example, in the cases of such important pieces of apparatus as the steam turbine, the electric locomotive, and in many applications of electric motors in steel-rolling mills and other large industrial operations. It is also necessary to investigate the electrical phenomena connected with the actual operation of long-distance transmission lines over great distances and high altitudes, and to observe the mechanical and electrical effects of short circuits in electrical systems carrying large amounts of energy, which may be only determined in the actual installation. Therefore, the industrial companies frequently have been forced to extend their research work outside of their own premises and make use, when permitted, of the facilities of large users of electrical apparatus. Progressive public utility companies and manufacturers have recognized the benefit of such research work, and assumed their full share of the responsibility and risk involved in utilizing experimentally, when needed, the apparatus and other facilities. They have done even more by employing engineers and research men to carry on much of this work at their own expense.

The research departments of universities have also taken up such field work, as an interesting example of which is the work of Professor Ryan at Stanford University in California, on the study of corona effects and the losses as influenced by shape and size of cable on long-distance transmission lines. This is one

of many illustrations of the overlapping of industrial and educational research in the experimental and industrial field.

The extent to which the movement for industrial research laboratories has grown in this country may be seen by consulting the bulletin of the National Research Council, which in 1921 published a list of over 500 industrial laboratories employing some 5000 chemists and physicists. This does not include laboratories connected with Federal, state or municipal governments, or with educational institutions.

It must not be supposed that the benefits of the discoveries of the industrial research laboratory are confined to the firm which bears the expense, the industry with which it is associated or even to the country in which it is located.

It is usually the policy of industrial companies not only to permit, but to encourage publication of their work by scientists in their employment. By this means, the entire industry and the world in general are informed, and diffusion of knowledge is increased.

As an instance, I may mention that there have been published by members of our organization, under the auspices of the Research Laboratory, during the past ten years, over 360 articles by seventy different men. These range from articles covering purely scientific research, such as "The Structure of the Helium Atom," to descriptions of new and useful processes or applications, such as "The Application of the Photo-electric Cell to Practical Photometry."

It may be of interest to mention that the General Electric Company, in the development of the electric locomotive for main line service, found it desirable to build its own test track, four miles long, at its works at Erie, Pennsylvania. This is equipped with ample power for operation of the heaviest locomotives, at speeds up to 100 miles per hour. On this track has been installed a new testing machine capable of measuring the vertical and lateral pressures exerted upon the track rails by each axle of locomotive or car, at any speed. This testing device has been called the "Otheograph," and described in the technical press by Mr. W. B. Potter, its designer. Its use has already given valuable experimental information upon a subject about which little is actually known. Tests have been participated in by the American

Locomotive Company and by the professors of the University of Illinois.

The industrial researcher must keep fully abreast of all the disclosures and publications made by similar workers, either in pure science or in technical development, whether in this or in other countries, as every new scientific discovery must be carefully considered. A scientific library is, therefore, an essential part of the laboratory equipment, and it is of greater importance that it should be fully used.

Mere reading is not sufficient. It is frequently desirable that the experiments, and to some extent the work of other researchers, should be repeated. It is necessary to have that intimate knowledge which comes from personal performance of the actual experiments. So it has been found useful to have as a part of the Research Laboratory a room in which may be set up duplicates of the latest or most important scientific apparatus, as, for example, Wilson's apparatus for showing the path of an alpha ray, or apparatus with which may be repeated such fundamental experiments as Millikan's determination of the charge on the electron, the fundamental unit of electricity. This equipment is available for the use of interested, qualified men, and is often used for educational purposes.

Research work is not always interesting to the outsider. It may easily appear most monotonous, as often experiment after experiment has to be repeated with only minute variations. Only the researcher may see its beauty and feel the joy of work well done when success crowns his efforts.

For instance, one of our research men has worked for twelve years on mixtures of tar, asphalt, pitch and gilsonite and, as a result, has developed the foundation for most of the insulating materials which are now in use in the winding of coils for our apparatus.

Again, two men have worked for ten years on improvements in carbon brushes for electrical machinery, not very interesting or beautiful objects, but of great importance in our industry, as any improvement or deterioration in quality of such brushes may easily affect the capacity and performance of such large apparatus as rotary converters, large industrial motors, or the countless motors used in transportation.

Doctor Whitney tells that "our research men are doing such

apparently fruitless things as measuring the rate of flow of a black oil into a white one, when the only cause for any motion at all is an electromotive force impressed on the oils. This is related to researchers on better high-voltage cables."

One can never tell in advance whether a new discovery or fact has any technical application of value. As one example of many, we may mention that Ramsey and Raleigh, about 1894, discovered certain gases in our atmosphere, among which was argon. These gases were chemically inert, and remained scientific curiosities for many years until Langmuir found that argon was the very thing that he required for use in his latest gas-filled incandescent lamp.

Also Laue, Bragg and Moseley discovered that each element gave out a characteristic radiation when subjected to X-ray bombardment, and found in this characteristic radiation the so-called "X-ray spectrum," a phenomenon of great scientific interest, yet which offered at first no field for commercial application. Now X-ray spectrum analysis forms a part of the daily routine of the scientific industrial laboratory. Such analysis is used to discover the internal structure of materials. It even discloses the mystery of the atomic structure. It is like an atomic microscope which enables us to see, for example, the effect of rolling or drawing upon the internal structure of metal, the changes produced by "fatigue" or strain, and the atomic structure of alloys. It has led to a beautiful explanation of the mysterious fact that the element carbon is hard in the diamond, and slippery lubricating material in graphite. It discloses "that in the diamond the carbon atoms are so located that each atom shares two electrons with each of its four neighbors in tetrahedral form, while in graphite carbon atoms are arranged in layers or separated sheets." Thus, we see from the nature of the molecular arrangement why the properties of carbon in the diamond and in graphite should be so different.

Scientists are now obtaining such definite information about atoms and molecules and their internal structures that they are learning to regard them as very real and definite things, differing not only in size but in form; some molecules are like marbles, some like bricks, others again like long rods, the dimensions of which are known with great accuracy.

In fact, we hear constant discussion of structures "one molecule deep," of so-called "molecular layers," similar to a brick

or cobblestone pavement, and such ideas do not appear to be vague speculations, but practical enough to serve as the foundation of real technical advances.

Take, for example, colloids, which are small particles suspended in liquid (but not dissolved). Colloids are the basis of a new japan varnish, where the japan particles are suspended in water instead of being dissolved in turpentine or in naphtha.

Doctor Whitney says, "Here the japan is merely a fine suspension in water, kept in a permanent state of fine division by a modern process of stabilization which owes its efficacy to the fact that a substance is put into the mixture which forms a molecular layer on the surface between the colloid particle and the water. One picture of this process is that the chemical compound, which may be called the anticatalyzer, is of such a nature that one part of its molecule dissolves in the colloid material, or adsorbs on it, and the rest of the molecule dissolves in the water. In other words, chemical fences are interposed between the two substances, so that neither of them can completely aggregate." This water japan effects a considerable saving in manufacturing cost and offers the added advantage of lack of that fire hazard involved in the use of the most volatile solvents.

Again, he says, "Now among the molecular-layer phenomena is that peculiar phenomenon of catalysis where the mere presence of a trace of some foreign substance makes some processes or reactions proceed which do not take place without the catalyzer, or at least, take place very slowly. More novel still are the cases of the anticatalyzers, where a small quantity of some substance, apparently by its mere presence, prevents or reduces the rate of a reaction which would proceed much more rapidly without it. In the oxidation of many oils and similar chemical compounds such antibodies are known, and the problem is to introduce the proper ones into the special oils needing them, and this without introducing other complications."

As an interesting example of the extent to which industrial research becomes a creator of new devices and new industries, it may be mentioned that in one large electrical corporation a recent survey disclosed that about 15,000 men were engaged almost exclusively in the manufacture of products which were the outcome of the research laboratory. The value of the annual output was conservatively estimated at $60,000,000.

At one time an estimate was made of the benefit which the public received as the result of research in but one industrial field, namely, that of the incandescent lamp used in electric lighting. It was estimated that in 1920 the American people paid about $500,000,000 for the electricity that it used for such lighting. At that time the tungsten filament Mazda lamp was in general use. The question is: How much more would it have cost the people for the light that they actually enjoyed in 1920 if the Mazda lamp had been no more efficient that the carbon filament lamp? The answer is indeed startling: It would have cost an additional $1,000,000,000 at the lowest estimate, a difference of over one billion dollars a year in the cost of such electric light used by the country as a whole.

The amount of this remarkable saving, I believe to be considerably underestimated. However, if we were to add to this amount the estimated value of the results already obtained in all the other manifold fields of electrical manufacturing, the achievement just mentioned would represent but a small portion of the benefit which the American people have received as the result of industrial research and its technical application.

Similar scientific methods of research have been applied by the public electric utility companies under the direction of their leaders in finance and engineering, and have been largely responsible for the remarkable growth of that business, and the unequalled service it renders the public. The methods and results achieved have been fully published on many occasions, especially by that great pioneer and leader, Dr. Samuel Insull.

History shows that industrial progress has been often dependent upon the energy and genius of some great individual. In the future advances will undoubtedly come in the same way, but we believe that more frequently the greatest progress will follow the use of organized research, and from coöperative efforts of men in the universities, industrial organizations, and national institutions. Science is international. Its field is the world. Information is freely exchanged, there is no tariff on scientific ideas, and progress is based upon the work of workers in every civilized land.

Rutherford says, "The unknown appears as a dense mist before the eyes of men. In penetrating this obscurity, we cannot invoke the aid of supermen, but must depend on the combined efforts of a number of adequately trained ordinary men of scien-

tific imagination. Each in his own special field of inquiry is enabled by scientific methods to penetrate a short distance, and his work reacts upon, and influences the whole body of other workers."

Is it not possible that there is a real danger that the wonderful success of science may be the means of her undoing? The constant succession of modern miracles is making us callous to their reality and value. May not our very familiarity breed indifference, if not contempt?

We have long lost the thrill inspired by the telegraph and the telephone, the airplane and the automobile, the X-ray and the electric light. Even the radio is a commonplace. We think nothing of hearing a human voice speaking to us across the sea.

Is it not desirable occasionally to stop our daily task long enough to review the wonderful record, to realize that our progress is not due to chance or lucky " rule-of-thumb " methods, but is the result of ceaseless activity and hard work by many earnest men, following the scientific method of research?

The scientific method has accomplished such wonders in industry that it is to be hoped that the value of its spirit and method will soon come to be more generally recognized and appreciated, and applied in other fields of human endeavor.

Therefore, in closing, I would inquire why should not the scientific method and spirit which has shown itself so effective and powerful when applied to material things, prove equally effective and powerful if applied to the relations of men to each other in organized society—in the economic and even the political arena?

It would appear to be a reasonable proposition that in such fields it is equally desirable that freedom from prejudice, careful observation, honest and truthful interpretation, the essence and spirit of scientific research, should be practised, and if adopted, that a basis would soon be discovered for that better understanding and knowledge needed for a true and lasting peace, and for the highest intellectual and spiritual development of man.

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[1] Quotations from address by Elihu Thomson, " Field of Research," August 21, 1899, publication *A.A.A.S.*

[2] Address by L. A. Hawkins, Research Engineer, General Electric Laboratory, *Proc. Canadian Elect. Asso.*

An Anomaly in Frictional Electricity. A. O. RANKINE. (*Proc. Phys. Soc.*, Vol. 36, Pt. 5, 1924.)—Two ebonite rods from the same piece were used. One retained the original surface, while the other had acquired a polish by being rubbed with Government flannel. Both of the rods became negatively charged when rubbed with the flannel in its ordinary state. The fabric was an imperfect insulator, but it became a good insulator by thorough washing and subsequent drying. In what follows the flannel was used in its highly insulating state. Friction between the unpolished rod and the fabric always causes the rod to become negatively charged. "If the flannel is suspended and beaten vigorously a number of times with the polished rod, the latter at first acquires the usual negative charge. When the beating is continued, however, the rod acquires eventually a large positive charge. (This disconcerting effect occurred in the first instance after the students had been confidently assured that the reverse would be the case.) The polished ebonite appears to pass gradually under the treatment from a negative to a positive condition with respect to the flannel. When the rod is rubbed lightly with the prepared flannel after the above treatment, it becomes negatively charged, but subsequent vigorous rubbing reproduces the positive electrification. It is clear that in frictional electrification attention has to be paid to the mode of execution; the constitution of the substances is not the sole determining factor in the distribution of the charges." G. F. S.

Some Properties of Single Metal Crystals. P. W. BRIDGMAN. (*Proc. Nat. Acad. Sciences*, Oct., 1924.)—Young's modulus, linear and cubic compressibility, linear thermal expansion and electrical resistance were measured. Direction within the crystal has a great influence on the elastic constants. The linear compressibility of tin parallel to the axis of rotational symmetry is not greatly different from that perpendicular to the axis, but it is quite otherwise with Zn, Bi, Sb and Te. "The linear compressibility of Zn is nearly seven times greater parallel to the axis than at right angles, and the linear compressibility of Te is negative parallel to the axis." Bi and Sb like Zn have much greater compressibilities parallel to the axis than at right angles to it. The corresponding constants for Cd were not obtained because two hitherto unknown polymorphic forms of the metal turned up under pressure. Deformability, linear thermal expansion and electrical resistance are greatest across the plane of easiest cleavage or slip. There is great difference in the linear thermal expansions parallel and perpendicular to the axis. For Zn this coefficient is 4.5 times as great parallel as at right angles to the axis. Te again distinguishes itself by having a negative coefficient for directions parallel to the axis. In the matter of electrical resistance it shows no similar comportment, though like the other metals it displays considerable difference between the values of its coefficients in the two directions. G. F. S.

RECENT PROGRESS IN THE SCIENCE OF AERONAUTICS.*

BY

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THE aeronautical engineer whose function is to design aircraft has, in the past, followed several methods, or combinations of methods.

One of these, the most obvious, is to rely largely upon past experience. One successful type leads to another, refined in some particulars, it is true, but essentially the same. This method was the one followed until very recently in the design of airships. As a matter of fact, there could be no other method until direct physical experiments were performed upon the distribution of pressures over the envelope, fins and elevators and rudder, and the stresses and strains in the members of the framework were measured. The defects of the method are sufficiently obvious, the main one being that ignorance of detailed knowledge would surely lead to making the structure too heavy or too weak.

The method most commonly used to-day is to obtain preliminary knowledge of the aerodynamical properties of a model of the aircraft or of models of its various parts. For this purpose, models, of possibly one foot or two feet in size, are made and tested in wind-tunnels. These last are circular tubes of large diameter, up to ten feet or more, through which a current of air may be drawn by fans at a velocity of 100 miles an hour or more. The model is suspended in the air-stream from a balance, to which the necessary weights may be added in order to keep the model stationary; and in this manner the forces and moments acting on the model under varying conditions may be determined. All the countries in the world have such wind-tunnels, and many manufacturers of aircraft have their own. Thus a designer of a new shape of airplane wing will have a model made as exactly as possible, and will then in the wind-tunnel test its lift and drag

* Address delivered in the hall of The Franklin Institute, Wednesday, September 17, 1924, on the occasion of the centenary of the founding of The Franklin Institute.

under different angles of attack, determine the motion of its centre of pressure, etc. All of these data are extremely useful. Again many experiments may be performed in wind-tunnels upon such problems as:

(*a*) The effect of the presence of the fuselage of an airplane upon the action of the propeller;

(*b*) The effect of giving different shapes to the trailing edge of a wing upon the distribution of force over wing;

(*c*) The relative aerodynamic advantages of biplane or monoplane construction for certain types of airplanes.

No one can overestimate the usefulness of all the information which has been obtained from observation upon models; but the method suffers from one fundamental weakness. It is not possible to deduce exact conclusions as to the aerodynamic properties of a full-scale aircraft from the knowledge of those properties for models of the aircraft. There are forces of two distinct types acting upon an aircraft when it moves through the air; one of these depends upon the inertia of the air, that is, upon the density of the air and its velocity relative to the aircraft; the other upon the viscosity of the air. When a model of an aircraft is placed in an air-stream, no change is made in the nature of the air, but since the dimensions of the model differ from those of the full-size aircraft, the relative importance of the two types of forces is changed. It has been known for many years, from certain quite elementary considerations, that the aerodynamic properties of aircraft and model agree only for one special condition. If V is the relative velocity between the air and the solid body, ρ is the density of the air, μ is its coefficient of viscosity and L is a *linear* dimension of the solid body (*e.g.*, the diameter of a cylinder or strut, the length of the chord or span of an airplane wing, the length of an airship, etc.), then the aerodynamic properties are the same if the fraction $\frac{\rho VL}{\mu}$ is the same for the aircraft in flight and the model in the wind-tunnel. This fraction is called the Reynolds number, and obviously is of fundamental

importance. In the use of a model, therefore, in an ordinary wind-tunnel, while V may approach that of the actual flight of the aircraft, and while ρ and μ are the same, L is much smaller, being perhaps one-twentieth for an airplane and one-three-hundredth for an airship. So it is apparent why the method itself is defective. In spite of this fact, and for reasons which need not be elaborated here, the method does yield important and useful results, and it has given, in the main, the store of data from which the designer of aircraft has drawn. It is seen at once that the method can be improved; can, in fact, be made theoretically ideal, if the density of the air used in the wind-tunnel is compressed as much as the linear dimension of the aircraft is diminished, for under these circumstances, the product ρL does not change, and it is known that μ, the viscosity, is not affected greatly. The difficulty of securing these ideal conditions is a purely engineering one; it is to place the entire wind-tunnel in the interior of a tank in which the air may be compressed as much as is desired. A wind-tunnel of this type has been made by the National Advisory Committee for Aeronautics of this country and is in operation at its laboratories at Langley Field, Virginia. With the results now being obtained in this laboratory, the aeronautic engineer may make his designs with absolute confidence.

Another method which has been perfected within recent years has been one consisting of making observations upon actual aircraft while in flight or in making manœuvres. (In this way, correction factors have been obtained which could be applied to data determined in wind-tunnel experiments upon models.) This method requires the operation of a large number of recording instruments for the accurate registration of velocities, accelerations, pressures, etc. By means of these, the actual motions of aircraft under varying conditions, of the distribution of pressure over all parts of their surfaces, of the motions of the control surfaces (rudder, ailerons, elevators), of the forces required of the pilot to secure results, etc., have all been determined to a high degree of accuracy. This information is especially important in helping the designer to make a proper distribution of his loads so as to give the proper strength to the structure and yet not to

increase the weight, and also to know exactly what are the essential conditions for safety in flight, for controllability, *i.e.*, ease of altering the state of flight, and for manœuverability, *i.e.*, rapidity of alteration of the condition of flight. The larger part of the improvements made in recent years in the design of both airships and airplanes is due directly to the information obtained by this experimentation with full-size aircraft.

Still another method is available to-day, which unfortunately has not yet been used extensively in this country but which was used by Germany with the utmost success in the war. This consists of the application of the theory of hydrodynamics to aeronautical problems. This subject has been perfected by the work of mathematicians and physicists, and forms one of the most beautiful illustrations of applied mathematics. The general theory is available to everyone in Horace Lamb's admirable treatise on hydrodynamics, which is a perfect storehouse of information. The difficulty in the past has been to apply it. Air, as a fluid, is compressible—as distinct from an ideal liquid; and it shows viscosity when one layer moves over another or when a solid body moves through it. These facts make it impossible, at present, to apply the equations of hydrodynamics to such a fluid; the mathematical operations are too difficult, except in a few comparatively unimportant cases. Careful consideration, however, shows that for many practical problems, of interest to aeronautical engineers, it is allowable to treat the air as of constant density and to neglect its viscosity. This last comes practically into account only in the immediate neighborhood of the surface of the aircraft and the compressibility of the air becomes an essential feature only when velocities of 1000 feet a second or 12 miles a minute are considered. In other words, for a large class of problems, air may be treated as an "ideal liquid." Attention was first drawn to this fact by W. Kutta and by L. Prandtl. But, granting it, great difficulties still remain. There is no general method, that is practicable, for treating mathematically the motion in a liquid of a solid body of any arbitrary shape. Actual aircraft, either airships or airplanes, have irregular shapes, fixed appendages and movable parts; so they cannot be discussed directly. The plan has been, therefore, to simplify the problem by selecting for mathematical treatment certain ideal shapes and by making, when necessary, certain physical assumptions. The general problem has been sim-

plified enormously by the recognition of the fact that in the actual flight of aircraft there are many conditions under which the air particles in any plane remain in that plane during flow, thus being an illustration of uniplanar flow (two-dimensional). The importance of this fact lies in the knowledge of a general method of the mathematical treatment of such motions. Thus when an airship is moving under pitch (or yaw), the portion of the airflow created by the pitch (*i.e.*, the transverse flow) near the centre of the ship is practically uniplanar, since there the airship has a cylindrical shape.

The flow about airships has been discussed in two quite distinct ways. Since the lines of flow of the fluid are along the surface of the airship, one mode of attacking the problem theoretically is to deduce such a distribution of sources and sinks along an axis as will give lines of flow which, when combined with a uniform flow—due to the motion of the airship—will give lines of flow lying in a surface identical with that of the airship. The whole character of the flow everywhere may then be deduced from the strength and distribution of the sources and sinks. This would then enable one to calculate the distribution of pressure over the surface of the airship. This method has been used with great success by G. Furhmann, who tried various distributions of sources and sinks, thus obtaining by trial various surfaces of flow which agreed as accurately as need be with the surfaces of actual airship types. The resulting pressures obtained by calculation were found to agree most closely with pressures actually observed, except at points near the stern where, of course, agreement was not expected, since it is at those points that the effect of viscosity in the surface layer of air makes itself felt most.

M. Munk has solved the same problem in the case of an ellipsoid of revolution by an ingenious deduction from a general formula of Lamb's concerning the flow past an ellipsoid. Munk's formula for the pressure applies also to an airship flying with an angle of yaw (or pitch).

Another interesting and important problem concerning airships has also been solved by Munk, using general mechanical principles. This is the determination of the unstable moment acting upon the airship when flying with an angle of yaw in either a straight or circling path. And, taking into account the fact of the uniplanar flow in central transverse planes if there is trans-

verse motion of the airship, Munk has established the distribution of transverse forces acting on the ship when in straight or circling flight, with a small angle of yaw. Knowing the unstable moment and the centrifugal force, in the case of circling flight, the properties of the fins, the stationary fixed surfaces attached to the airship near its stern, may be discussed. In short, Munk's formulas are absolutely essential for the designer of airships and furnish information which could otherwise be obtained only by most elaborate observations.

The theory of airplane wings is divided naturally into two parts, that concerned with the effect of the cross-section of the wing, its profile, and that which takes into account the effect of the tips of the wing. The former really treats the wing as if it had a span of infinite length, and the flow is, then, strictly uniplanar. The problem of the flow in this case was first successfully attacked by W. Kutta and by N. Joukowski. Kutta was the first to show that in order to have a transverse force act on the wing, that is, in order to have a lift, it was essential that part of the flow about the wing should be a circulatory flow around it, and he deduced the formula, bearing his name, giving the connection between the amount of the circulation of this flow and the lift. His method then was to deduce the combination of two flows about a circle—the problem being a uniplanar one, one a uniform flow in a certain direction, the other a circulatory flow in circles around the original circle; then by means of a conformal transformation to transform the original circle into some other closed curve and the original lines of flow into lines of flow around this new section; finally so to choose the relation between the velocity of the uniform flow and the circulation in the original problem that in the new one the total resultant flow divides exactly at the "trailing edge" of the section. Following this process Joukowski was able to obtain certain closed curves which resembled greatly some well-known wing profiles, and thus to deduce the nature of the flow about them, the distribution of pressure over the wing, etc. This process of Kutta can be applied to only a limited number of cases; and evidently it cannot be used to deduce the flow about a wing whose section is given arbitrarily.

Munk has developed a much more general method for the discussion of the infinite wing, and has deduced general formulas for the lift, the pitching moment and the position of the centre

of pressure, for a thin wing of any cross-section. He first shows that, as far as lift and pitching moment are concerned, the flow about the thin wing may be considered—to a good approximation—the same as the flow about a curved line (the flow being two-dimensional) which is the medial line between the upper and lower surfaces of the wing, having the direction at any point of the element of the wing. He next shows how Kutta's theorem concerning the lift, combined with his method of treating the flow at the trailing edge, leads to a theorem connecting the lift with the distribution of transverse velocity component along the chord of the wing. This last evidently depends upon the slope of the wing curve at each point. So ultimately, Munk obtains his formulas for lift and moment expressed in terms of the geometry of the wing curve. If the coördinates of this curve are known, as they are in practice, the values of the lift and moment may be deduced at once. These formulas of Munk may be used to discuss the general properties of any type of wing, without the need of awaiting tests in a wind-tunnel.

In order to describe the effect of the ends of a wing, Prandtl, calling attention to the fact that there must be a circulation around the wing in order to secure lift, compared the wing to a portion of a vortex filament whose two end-portions run off perpendicularly backward from the two wing tips. The velocity of flow due to this vortex would account in a general way for the change in the angle of attack, *i.e.*, the "induced" angle of attack, for the induced drag and for the effect of one wing upon another in a biplane or multiplane. Further, assuming that the strength of the vortex varied along the span, so that the density of lift along the span varies and that vortex filaments run backward off the wing at all points, a still better approximation to reality was secured. By making a proper hypothesis as to the distribution of the lift along the span, definite formulas could be deduced. When these formulas were compared with the results obtained in wind-tunnels, very interesting conclusions could be drawn as to the connection between induced drag and the drag due to viscosity. Prandtl also indicated the solution of the problem which arises, when, instead of having given the distribution of lift, the distribution of induced downwash along the span is specified; and A. Betz solved a special case of the general problem of determining the distribution of lift for a wing of given plan form and a given

angle of attack. When Prandtl applied his concept to biplanes, he was able to deduce most interesting theorems, all of importance to designers.

Munk's treatment of the same problems as just discussed, dealing with the finite wing, is much simpler than Prandtl's, involving, as it does, only the ideas of energy and momentum. His general thought is again to discuss uniplanar motion, this time in a transverse layer perpendicular to the line of flight, through which the wing passes. In this case, the front view of a monoplane can be considered a straight line having a length equal to the span, and the uniplanar flow is about this line in a vertical plane. Munk deduces the induced angle of attack and the induced drag, when the span is given, and, also, what is most important, the effect of induction, that is, of the flow itself, in modifying the effective angle of attack, both so far as concerns the lift and as affecting the rolling moment. In short, Munk's method gives exactly the formulas needed by the aeronautical engineer.

Similarly, when applied to biplanes, with or without stagger, Munk's mode of approach is distinctly physical, rather than mathematical, and leads directly to the solution of the problems concerning lift, induced drag, moments and position of centre of pressure.

As a consequence of this theoretical work of Prandtl, Betz and Munk, the science of aeronautics has at its disposal certain formulas which enable one to calculate the principal properties of an airplane wing of given profile and dimensions and also of any combination of wings. Furthermore, these formulas make it possible to discuss the effect of modifications in the wings, as, for instance, of changing the ailerons. It is true the formulas are based upon simplifying assumptions; but the results obtained from them are of the same order of accuracy as those obtained from tests in wind-tunnels.

Photochemical Properties of Cuprous Oxide.—ALLEN GARRISON, of the Rice Institute (*Jour. Phys. Chem.*, 1924, **28**, 279–284), finds that white light has no influence on the conductivity of a suspension of cuprous oxide in pure water, but increases the conductivity of a suspension of copper coated with a thin layer of cuprous oxide, the photo-potential depending on the amount of copper. The system—cuprous chloride-cuprous oxide-copper-water—has a higher resistance in the light than in the dark, affording a new example of a consecutive photochemical reaction. J. S. H.

REFRACTOMETRIC STUDIES OF NEUTRALIZATION.*

BY

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It is well known that the refractive index of substances in solution is practically a linear function of the concentration. The factors which tend to impart curvature to the straight line representing the relationship have been discussed by Baxter and his co-workers.[1] Since these factors are of minor significance from the point of view of this study, they will be neglected and it will be assumed that a straight line is an adequate representation of the relation. The literature contains many reports which demonstrate the validity of the assumption for pure solutions and for solutions of mixtures of non-reacting compounds. References to some of these are to be found in a previous publication.[2] On the other hand, little, if any, information is to be had of the course of change in refractive index during acid-base neutralization. It is true that Doerr and Berger[3] mixed equal volumes of 0.01 N hydrochloric acid and 0.01 N sodium hydroxide and were able to account for the change in refractive index on the basis of the sodium chloride concentration (0.005 N), but this is as far as their studies in this direction were carried.

It seemed worth while to make a refractometric study of the processes of neutralization both from the theoretical and from the practical point of view. From the theoretical, in that extension of the generalization or disclosure of new relationships was assured; from the practical, in the hope that the findings would assist in the determination of how far refractometric measurements are useful in the quantitative analysis of mixtures of chemically reacting substances in solution.

The problem has several points of approach, two of which were chosen. The one is the influence of the strength of the acid (ionization); the other, the influence of the basicity of the acid on the nature of the change in refractive index during neutralization by strong alkali.

* Communicated by the Author.

PROCEDURE.

Normal solutions of hydrochloric, acetic, sulphuric, oxalic, phosphoric and citric acids were prepared, and mixed as follows with normal solutions of sodium or potassium hydroxide. For each series of tests an array of nine test-tubes was placed in a rack. In the first tube there were placed 9 c.c. of acid and 1 c.c. of alkali; in the second 8 c.c. of acid and 2 c.c. of alkali, and so on to the ninth tube, which contained 1 c.c. of acid and 9 c.c. of alkali. In some series intermediate mixtures were made, the need for which will be shown in the discussion. In addition to the neutralization series there were prepared for each acid and alkali dilutions representing 0.75 N, 0.50 N and 0.25 N concentrations, and from the 5 : 5 acid-alkali mixtures dilutions representing 0.375 N, 0.250 N and 0.125 N concentrations of the neutral salts from which the refractive index-concentration relations of the respective compounds as such were determined. The solutions were made up of the ordinary c. p. chemicals without special purification, and distilled water. The refractive index of the water was determined in each test. It ranged from 1.33283 to 1.33307. In most of the tests it was 1.33299, the same value as that reported by Baxter, Burgess and Daudt.[4] All measurements were made with a Pulfrich-Zeiss refractometer at 20° C., using sodium light.

RESULTS.

The observations are given in charts instead of tables. The former yield adequate information and save space. The latter would add nothing of value to the presentation since the absolute values do not lend themselves to any more detailed interpretation than that given. In the charts the ordinates give the mixtures in c.c. of acid and alkali, the abscissæ the refractive index of the mixtures minus that of the water in the particular test, times 10^4. In this way simplification of presentation is attained.

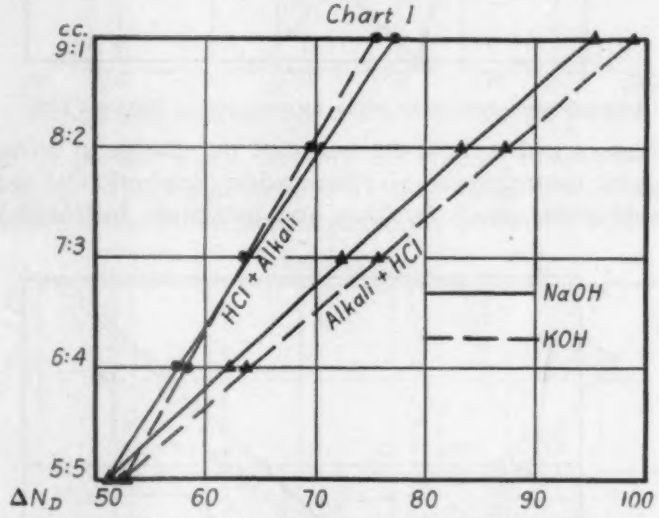
Before going on to a discussion of the results, attention should be called to the fact that each series represents an arithmetical progression of increasing and decreasing concentrations of salt and acid, and alkali and salt, respectively, as follows:

Acid Concentration.						Alkali Concentration.					
N	0.8	0.6	0.4	0.2	0.0	0.0	0.2	0.4	0.6	0.8	M
N	0.1	0.2	0.3	0.4	0.5	0.5	0.4	0.3	0.2	0.1	M

Salt Concentration.

Bearing in mind the fact that the products of the individual terms of such a progression by a constant themselves form an arithmetical progression, and the fact that the sums of the corresponding terms of two dissimilar progressions is likewise an arithmetical progression, it is obvious that the course of the refractive index-concentration relation exhibited by any of these series should be a continuous straight line making an acute or obtuse angle with the base. This is what theory demands. Divagations therefrom indicate interfering factors.

Chart 1 shows that theory and observation coincide when

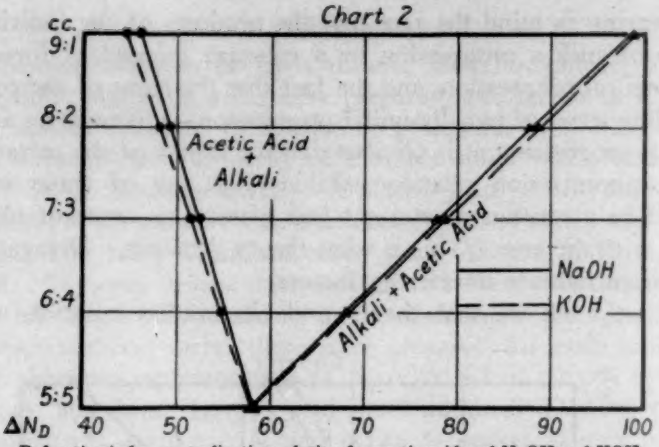


Refractive index-neutralization relation for HCl and NaOH and KOH.

strong mono-basic acid (hydrochloric) is neutralized by strong mono-acidic base (sodium or potassium hydroxide) and *vice versa.*

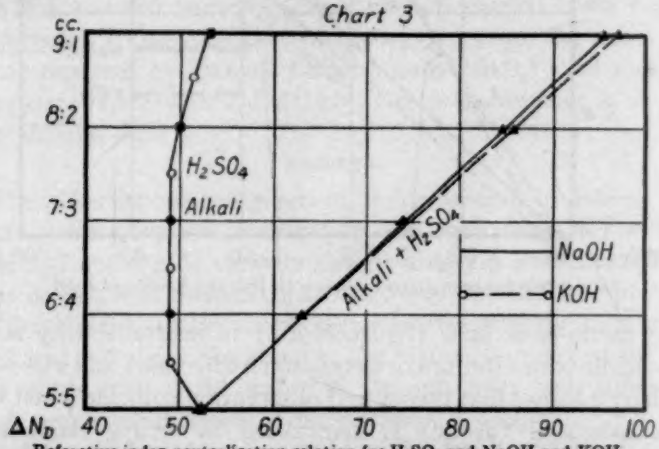
Chart 2 shows that theory and observation coincide when weak mono-basic acid (acetic) is neutralized by strong mono-acidic base (sodium or potassium hydroxide) and *vice versa.*

It is evident that strength (ionization) of acid is of negligible influence in the additive function of the refractive index as related to concentration changes during neutralization. Hence extension of the generalization to chemically reacting mixtures of strong and weak mono-basic acids and strong mono-acidic bases is allowable.



Refractive index-neutralization relation for acetic acid and NaOH and KOH.

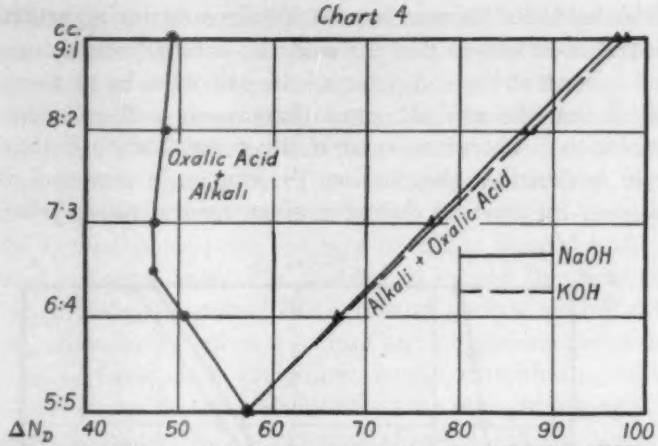
Charts 3 and 4 show the course of the change in refractive index with neutralization of dibasic acids (sulphuric and oxalic) by mono-acidic alkali (sodium and potassium hydroxide) of



Refractive index-neutralization relation for H_2SO_4 and NaOH and KOH.

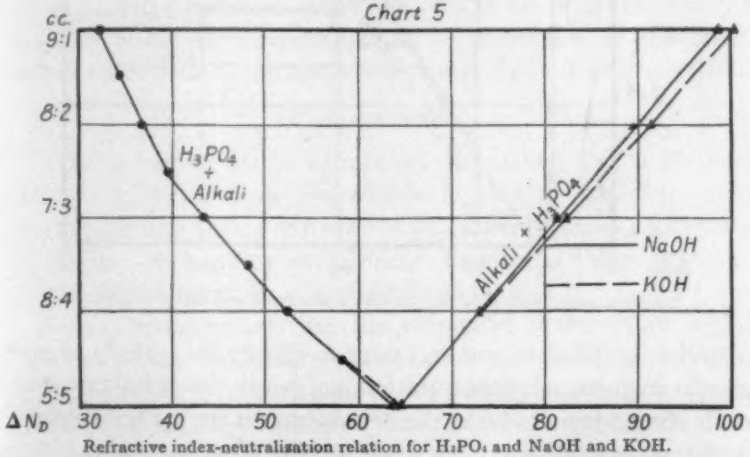
equivalent concentration, and the same when alkali is neutralized by acid.

When dibasic acid is neutralized by alkali, the refractive index-neutralization relation is not a continuous straight line. The same is true when tri-basic acid (phosphoric and citric) is similarly studied, as seen in Charts 5 and 6. Apparently an interfering



Refractive index-neutralization relation for oxalic acid and NaOH and KOH.

factor is present which prevents theory and observation from coinciding. This factor must be a change in the relative rates of formation of the different salts potentially producible in the given

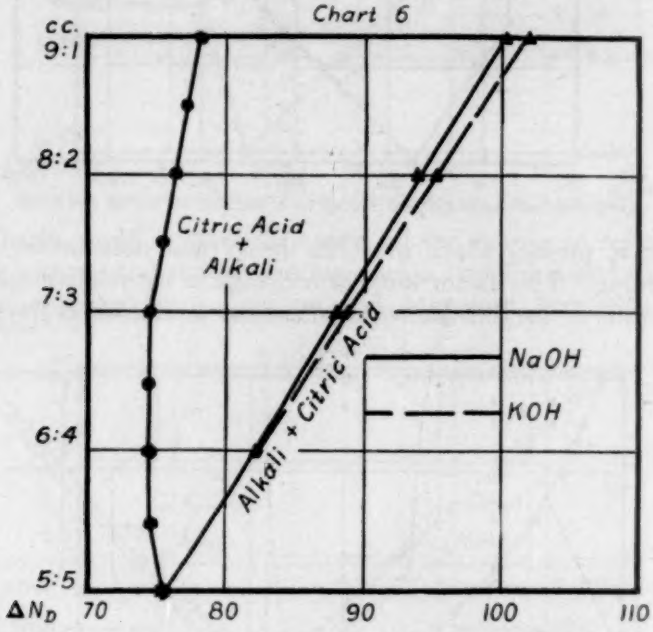


Refractive index-neutralization relation for H_3PO_4 and NaOH and KOH.

series under the conditions of procedure. It is improbable that the residual free acid after neutralization of a definite part of the initial concentration changes its nature in any way detectable by refractometry.

Now the theory as postulated demands an arithmetical progression in acid and salt concentration. In the case of dibasic

acids this holds for the acid but not for the salts during neutralization. It is well known that the acid salt is first formed, then the normal. Since at the end point all the salt must be as normal, it is evident that the acid salt must decrease as well as increase in concentration. Therefore even if the concentration changes do occur in arithmetical progression, the change in direction of the progression for one salt during a given neutralization procedure



Refractive index-neutralization relation for citric acid and NaOH and KOH.

is all that is sufficient and necessary to distort the orderly progression in uniform salt concentration and hence the refractive index.

It should be noted that the neutralization curves for sulphuric, oxalic, phosphoric and citric acids are the same for sodium as for potassium hydroxide. The intermediate points on these curves were determined in order to verify the distortion from the normal. In the case of oxalic acid, neutralization with potassium hydroxide resulted in precipitation in the 9:1 and 8:2 mixtures, the values on the chart at these points are for sodium hydroxide neutralization only.

Turning to the course of the refractive index-neutralization relation when alkali is neutralized by dibasic and tri-basic acid, it is seen that observation consistently coincides with theory.

This shows in a new fashion that the neutralization of base by acid is quite a different process than neutralization of acid by base with respect to the nature of the salt mixture formed, when the acids in question are di- or tri-basic. It leads to the idea that in the former process but one type of salt is formed, and that the normal. At any rate, it is possible to extend the generalization stated in the early part of this report to include neutralization of strong mono-acidic alkali (sodium and potassium hydroxide) by di- and tri-basic acids (sulphuric, oxalic, phosphoric and citric). Which is to say that the exhibition of the straight line relationship between refractive index and neutralization (by di- and tri-basic acid) indicates that the resultant products change in concentration in arithmetical progression. A phenomenon which is not exhibited when the same acids are neutralized by mono-acidic bases.

These observations also demonstrate that the refractive index of an acid-salt mixture, when the acid is di- or tri-basic and the salt the result of a neutralization of a portion of the acid by mono-acidic base, is no measurement of the salt concentration.

CONCLUSIONS.

The refractive index of mixtures of strong and weak mono-, di- and tri-basic acids (hydrochloric, acetic, sulphuric, oxalic, phosphoric and citric) and strong mono-acidic bases (sodium and potassium hydroxide) is a linear function of the process of neutralization when base is neutralized by acid.

The generalization that the refractive index is an additive function of the molecular concentration of substances in solution is thus extended to mixtures of chemically reacting compounds of this type, and the data show that the resultant products change in concentration according to the law of arithmetical progression.

The same is true for the neutralization of strong and weak mono-basic acid by strong mono-acidic base.

The refractive index of mixtures of di- and tri-basic acids and mono-acidic bases is not a linear function of the process of neutralization when the acid is neutralized by the base. Hence the generalization is not applicable to chemically reacting mixtures

of this type, and the data show that the change in concentration of the resultant products does not follow a simple law of arithmetical progression.

The refractive index of an acid-salt mixture of this type can not be used as a measurement of the salt concentration.

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Brownian Movements of the Same Particles under Different Gas Pressures. S. LANDMAN. (*Zeit. f. Physik*, Vol. 27, No. 4, 1924.)—A particle in a liquid or a gas is buffeted on all sides by the adjacent molecules and in consequence has a motion as irregular as that of a punching-bag knocked in all directions by a group of enthusiastic and tireless athletes in training. When this phenomenon was first observed about a century ago, it gave no promise of making any substantial contribution to science. It was interesting but little more, yet few chance discoveries have led to more fruitful interpretations. In the hands of Perrin the study of the Brownian Movements have furnished valuable data respecting the movements of molecules and an additional means of determining Avogadro's Number. Before the publication of this paper observers have been content to observe the irregular movements of particles subject to no change of pressure. Here a new department is made and the same particle is held under scrutiny while the pressure in the surrounding gas is varied through a considerable interval. Drops of oil between the horizontal plates of a condenser were raised by the application of the electric field and permitted to fall while under observation through a microscope. This could be done again and again and at the same time the pressure of the gas could be changed, or even a different gas substituted for the first. Take particle No. 130 as an example. Under a gas pressure of 745.7 mm. of mercury a million times the mean square of the distance through which it was displaced in a second in its Brownian Movement equalled .537 cm. The corresponding numbers for pressures of 131.8, 60.0, and 28.1 mm. are .950, 1.856 and 5.246. Up to this point the gas in the case was air. At a pressure of 37.85 mm. nitrogen was admitted and the corresponding length was 2.924 cm. With nitrogen at 743.1 mm. it returned to almost the original value, .607. "It appears, therefore, as was to be expected, that with diminishing pressure Brownian Movements increase in liveliness." G. F. S.

NOTE ON DR. LOUIS COHEN'S PAPER ON ALTERNATING CURRENT CABLE TELEGRAPHY.*

BY

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RECENT numbers of this JOURNAL contain two papers by Dr. Louis Cohen on the theory of electric transients on smooth lines and on the theory of cable telegraphy.[1] In the second of these papers Cohen states that certain parts of H. W. Malcolm's well-known book, "The Theory of the Submarine Telegraph and Telephone Cable," are incorrect. It is the purpose of this note to show that these criticisms are unfounded and to show that Cohen's own treatment of the subject is seriously defective in several respects.

The problems studied by Doctor Cohen are those of determining the instantaneous current (transient current plus steady state current) at the receiving end of a telegraph cable due to a suddenly applied sinusoidal electromotive force at the sending end of the cable. In one problem the cable is short-circuited at both ends, in the other the ends are closed through equal condensers. In solving these problems Cohen makes use of the formal symbolic methods of Heaviside, and he obtains formulas which give the correct solution in an abstract mathematical sense. However, when he changes these formulas into the forms which are directly applicable to physical problems, he neglects to take account of one of the most important of the conditions that affect the forms of the transient currents. Consequently his results in their final forms are valid for one special case only, and not at all for the general case as he implies. This error is discussed in the next paragraph.

It is intuitively evident, and mathematical calculation confirms the intuitive judgment, that *the nature of the transients will depend very essentially on the phase in which the sinusoidal*

* Communicated by Dr. H. D. Arnold, Director of Research.

[1] (1) "Electrical Oscillations on Lines," JOUR. FRANK. INST., Jan., 1923; (2) "Alternating Current Cable Telegraphy," JOUR. FRANK INST., Feb., 1923.

electromotive force is applied. That is, if the electromotive force is

$$E(t) = 0 \text{ for } t < 0,$$
$$= E \sin(\omega t + \theta) \text{ for } t > 0,$$

the nature of the transients will depend on the value of the phase angle θ. *Nevertheless, Cohen nowhere makes any mention of the phase angle.* He carries through the calculations as if $\theta = \frac{\pi}{2}$, and his results seem to be correct for that case (except in one particular to be mentioned presently), *but he states his results as if they were universally valid for all cases in which the applied electromotive force is a sinusoid of frequency* $\frac{\omega}{2\pi}$. This is the more remarkable because the practical application of his theory that Cohen has in view is Squier's system of sine-wave telegraphy, where it is essential that the electromotive force be applied so that $\theta = 0$. Cohen's results, because they are only valid for the case $\theta = \frac{\pi}{2}$, are really not applicable at all to this system of telegraphy; while those of Malcolm are applicable, as will be shown later.

In the case of a cable of length l, of total resistance and total capacity Rl and Cl, respectively, without terminal impedances, the steady state current at one end due to the applied electromotive force $E e^{j\omega t}$ at the other end is

$$I_s = \frac{E\sqrt{\frac{j\omega C}{R}}}{\sinh(l\sqrt{j\omega RC})} e^{j\omega t}. \tag{1}$$

This formula is correctly given by Cohen (second paper, first of equations (26)). The steady state current due to the electromotive force $E \sin(\omega t + \frac{\pi}{2})$ is obtained by taking the real part of (1), it is

$$I_s = E\sqrt{\frac{\omega C}{R}} \frac{\cos\left[\omega t + \arctan \frac{1 - \coth\left(l\sqrt{\frac{\omega RC}{2}}\right) \cdot \tan\left(l\sqrt{\frac{\omega RC}{2}}\right)}{1 + \coth\left(l\sqrt{\frac{\omega RC}{2}}\right) \cdot \tan\left(l\sqrt{\frac{\omega RC}{2}}\right)}\right]}{\sqrt{\sinh^2\left(l\sqrt{\frac{\omega RC}{2}}\right) + \sin^2\left(l\sqrt{\frac{\omega RC}{2}}\right)}} \tag{2}$$

Cohen makes some error in taking the real part of the complex expression and gets the incorrect result

$$I_s = E\sqrt{\frac{\omega C}{R}}\,\frac{\cos\left[\omega t - l\sqrt{\frac{\omega RC}{2}} + \frac{\pi}{4}\right]}{\sinh\left(l\sqrt{\frac{\omega RC}{2}}\right)},$$

(second paper, first of equations (27)). Consequently all of his numerical computations relating to the steady state current are wrong. His evaluation of the transient current is correct, provided it is understood that $\theta = \frac{\pi}{2}$.

The first criticism of Malcolm is contained in the following sentences which appear on page 171: "The formula for I_t given by (26) agrees with the one obtained by Malcolm by a different method, and given in his book, equation (9), page 363. Malcolm made the error, however, in taking the imaginary part of the equation for calculating the received current instead of the real part as he should have done. This, of course, vitiates all his numerical calculations."

These remarks arise out of a complete misapprehension of the significance of the use of the complex electromotive force $E\ e^{j\omega t}$ which is closely connected with the neglect of the phase angle θ. If the e.m.f. is of cosine form, the current to which it gives rise is obtained by taking the real part of the complex current due to the electromotive force $E\ e^{j\omega t}$, just as the electromotive force $E\cos\ \omega t$ is the real part of $E\ e^{j\omega t}$. On the other hand, the current due to $E\sin\ \omega t$ is obtained by taking the coefficient of j in the same complex current, because $E\sin\ \omega t$ is itself the coefficient of j in $E\ e^{j\omega t}$. Now on page 363, immediately before the equation that Cohen is criticizing, Malcolm states explicitly that he is considering the electromotive force $E\sin\ \omega t$. Hence, he is right in taking the imaginary part of the complex current, and Cohen's criticism is without foundation.

His other criticism of Malcolm is found in the following sentences which appear on page 177: "Doctor Malcolm utilizes the same method in getting the solution of this problem, but he made the serious error in applying the expansion formula as given

by Heaviside for a constant e.m.f. to the condition of sinusoidal e.m.f. He modified the formula arbitrarily by the factor

$$\frac{1}{\frac{\omega CR^2}{p^2}+\frac{p^2}{\omega CR^2}}$$

for which there does not seem to be any foundation in theory. His conclusions, therefore, in this case are of course wrong."

Let us explain the point at issue a little more fully. Both Malcolm and Cohen are here studying the telegraph cable with equal condensers at the two ends. A sinusoidal e.m.f. is suddenly applied at one end of the cable, and it is required to find the instantaneous current at the other end.

When the electromotive force

$$\begin{aligned} E(t) &= 0 \quad \text{for } t < 0 \\ &= Ee^{j\omega t} \text{ for } t > 0 \end{aligned}$$

is applied to a circuit of impedance $Z(j\omega)$, the instantaneous current is

$$I = \frac{Ee^{j\omega t}}{Z(j\omega)} - \sum_{p_1}^{p_m} \frac{Ee^{p_n t}}{(j\omega - p_n)Z'(p_n)} \qquad (3)$$

where $p_1, p_2, \ldots p_n, \ldots p_m$ are the natural complex frequencies of the circuit.[2] The formula (3) is the generalized Heaviside expansion.[3] The ordinary Heaviside expansion,

$$I = \frac{E}{Z(0)} + \sum_{p_1}^{p_m} \frac{Ee^{p_n t}}{p_n Z'(p_n)}, \qquad (4)$$

which applies when

$$\begin{aligned} E(t) &= 0 \text{ for } t < 0 \\ &= E \text{ for } t > 0, \end{aligned}$$

follows from (3) by simply setting $\omega = 0$.

Now let us suppose that the circuit is such that all the natural complex frequencies are real. The system under discussion by Malcolm and Cohen satisfies this condition. Then all the terms

[2] By *natural complex frequencies* we mean the values of $j\omega$ for which $Z(j\omega)$ vanishes.

[3] For derivations of this formula see: Carson, *Phys. Rev.*, 1917; Fry, *Phys. Rev.*, 1919.

of (4) are real, and the coefficient of j in the transient part of (3) is

$$\sum_{p_1}^{p_m} \frac{E\omega}{p_n^2+\omega^2} \cdot \frac{e^{p_n t}}{Z'(p_n)} . \tag{5}$$

The expression (5), of course, represents the transient current due to the sudden application of the electromotive force $E \sin \omega t$. *It is obvious that we can obtain* (5) *from the transient part of* (4) *by multiplying each term* $\frac{Ee^{p_n t}}{p_n Z'(p_n)}$ *by the modifying factor*

$$\frac{p_n \omega}{p_n^2+\omega^2} . \tag{6}$$

Equation (3) is given by Cohen so we may say that the modifying factor (6) is an immediate consequence of the theory as given by him.

Malcolm gives the following expression for the transient current at the receiving end of the cable with equal terminal condensers when the e.m.f. $E \sin pt$ is suddenly applied

$$\sum_x \frac{E \exp\left(-\frac{x^2 t}{KRl^2}\right)}{Rl\left[\frac{\sin x}{x} + \frac{1}{x}\frac{Kl}{K_r}\left(\frac{1}{\sin x} - \frac{\cos x}{x}\right)\right]} \cdot \frac{1}{\frac{pKRl^2}{x^2} + \frac{x^2}{pKRl^2}} .$$

(Expression (17), page 371, Malcolm's book.) He gives the ordinary Heaviside expansion for this system in the form

$$\sum_x \frac{E \exp\left(-\frac{x^2 t}{KRl^2}\right)}{Rl\left[\frac{\sin x}{x} + \frac{1}{x}\frac{Kl}{K_r}\left(\frac{1}{\sin x} - \frac{\cos x}{x}\right)\right]} .$$

(Equation (16), page 370.) Hence his expression for the transient due to the sinusoidal e.m.f. may be obtained from his expression for the transient due to the constant e.m.f. by multiplying each term by the modifying factor

$$\frac{1}{\frac{pKRl^2}{x^2} + \frac{x^2}{pKRl^2}} .$$

Changing to Cohen's notation, we have

$$p = \omega,$$

$$\frac{x^2}{KRl^2} = -p_n;$$

and the modifying factor is

$$\frac{-1}{\frac{\omega}{p_n}+\frac{p_n}{\omega}} = \frac{-p_n\omega}{p_n^2+\omega^2}.$$

Thus the modifying factor actually used by Malcolm not only has a sound theoretical basis, but also it can be derived (except for sign) directly from Cohen's equations. As for the residual difference of sign, it does seem that there Malcolm is in error. He does not actually give the derivation of his formula, so it is difficult to determine just how the difference arises. However, the negative sign is a matter of little importance, for it has the effect only of inverting the transient current-time curve.

The source of Cohen's confusion seems to be in the fact that he forgets that there is a different modifying factor for each p_n—that is, for each component of the transient—and not a single multiplier affecting the entire formula.

It must be noted that although Malcolm and Cohen are virtually using the same formula in this problem, their numerical results cannot be compared, for Malcolm is considering the electromotive force $E \sin \omega t$ and Cohen's results are only correct when the electromotive force is $E \cos \omega t$.

In conclusion it may be worth while to say a few words on the present status of the theoretical study of transient phenomena.

During recent years the entire theory has been placed on a firm mathematical basis and has been brought into close connection with the general theory of analytic functions by the work of several different investigators.[4] It would seem that it is advisable to use these standard methods in preference to the formal symbolic methods of Heaviside which are no easier to follow or to apply than the standard methods and which are usually used (as by Doctor Cohen) with little or no attempt at justification. Of course a large part of Heaviside's work has been rigorously

[4] (1) Wagner, *Archiv. für Electrotechnik*, IV Band, 1916, 5/6 Heft.
(2) Bromwich, London, *Proc. Math. Soc.*, (Sec. 2), 15, 401, 1917.
(3) Carson, *Phys. Rev.*, 1917.
(4) Bromwich, *Phil. Mag.*, London (Sec. 6), 37, 407, 1919.
(5) Fry, *Phys. Rev.*, N. S., Vol. xiv, No. 2, 1919.
(6) Carson, *Trans. A. I. E. E.*, 1919.
(7) Fry, *Annals of Math.*, Vol. xxii, March, 1921.
(8) Carson, *Bell System Tech. Jour.*, Nov., 1922.

justified, but in order to understand the justification the student must become familiar with standard mathematical processes which can themselves be applied directly to the study of transients with no greater labor and with less chance of error. It is worth noticing that the symbolic methods did not always lead to correct results even in the hands of Heaviside himself.[5]

The two circuits considered by Doctor Cohen are such that the natural complex frequencies can be found easily, in one case by an analytic process, in the other by a simple numerical calculation. The theory of such systems has been studied very extensively, and little remains to be done beyond the solution of routine problems. On the other hand, much remains to be done in the case of systems where some of the natural complex frequencies are considerably damped (but not overdamped).

The Light of the Night Sky. LORD RAYLEIGH. (*Proc. Royal Soc.*, A 736.)—The sky at night was observed through three light filters which permitted the passage of these three spectral regions: (1) Red, orange and yellow, (2) green, close to the auroral line, (3) blue-green and blue. For a standard of comparison Lord Rayleigh used the light spontaneously emitted by potassium-uranyl sulphate under the bombardment of its own radio-active content. "The intensity of the night sky, as seen through the filter, is to be matched against a standard, so as to determine its variations from time to time. The intensity is in all cases below the limit for which color is perceptible, so that no difficulty of comparison arises from color differences. To my eyes, at all events, everything is gray under these conditions."

At intervals from October to May, 1924, observations were carried out. The red intensity was greatest on October 31st, when it had the arbitrary value of .50, and least on the succeeding dates of December 3rd and 4th, when it fell to .13. The ratio is 3.84. Similarly for the region of the green line the greatest and least values noted were October 31st, 1.0, and December 2nd, .26, of which the ratio is 3.84. "It appears that each of the three spectral regions examined is subject to considerable variations of intensity, both absolute and relative to either of the others." There were, for example, two dates about four months apart in time on which the blue intensity was the same for both, but for the second date the red intensity was 44 per cent. and the auroral intensity 50 per cent. of the value on the previous night. A study of the atmospheric transmission for various wave-lengths showed that "these ranges are trifling compared with those given above for the night sky. I have not yet obtained any

[5] *Cf.* Carson, *Bell System Tech. Jour.*, Vol. i, No. 2, p. 52.

trustworthy indication of a change of intensity in any of the elements measured in the course of a single night." The investigator continued his work in Italy, but the results obtained there seemed the same as those got a few days earlier in England. Sir Arthur Schuster made similar observations in South Africa. These rather indicate "that the auroral light may be a little brighter in South Africa than in England at the same time."

From Lord Rayleigh's labors, now continued for several years, it is clear that the auroral phenomena on the earth fall into two groups, (*a*) the polar aurora, showing the green line and also the negative bands of nitrogen; generally confined to high latitudes; frequently manifesting arcs and draperies; varying its appearance in a minute or less, and (*b*) the non-polar aurora, having the green line but no negative bands; occurring at all latitudes; alike all over the sky; changing little over considerable periods. The author ventures the suggestion that the non-polar aurora "may be a phenomenon of phosphorescence, the luminosity being excited by the sun during daytime and carried round by the earth's rotation as in the phosphoroscope.

"The light of the night sky is much richer in red, measured relative to blue, than is daylight." G. F. S.

A Method of Getting Large Single Crystals of Metals. L. OBREIMOW and L. SCHUBNIKOW, Petersburg. (*Zeit. f. Physik*, Vol. 25, No. 1.)—Commercial metals are aggregates of small crystals, because of the probable presence in the molten liquid of many centres of crystallization. This suggestion is confirmed by the impossibility of cooling molten metals, with the exception of antimony, below their melting points. In recent years large single crystals of metals have proved of interest on account of the difference between their physical properties and those of the same metals in commercial form. For example, crystals of pure zinc are more plastic than lead. A load of 35 g. per sq. mm. produces permanent increase of length, while they can be extended to five times their original length without breaking.

By the method to be described single crystals in the form of rods were grown. Diameters ranged from 2 to 10 mm., and lengths up to 30 cm. The metal to be crystallized was contained in a glass tube drawn out at the lower end into a capillary tube. The metal was melted in an electric furnace. The capillary was the first to cool and there crystallization began. Thence it spread upward into the rest of the liquid which was gradually reduced in temperature. All the enclosed liquid solidified into the same crystal to which an arbitrary form was imparted by properly selecting the shape of the container. The same size of capillary tube will not answer for all metals. For zinc the diameter must not exceed 1.5 mm., else more than one crystal will form. The reliability of the process is indicated by the statement that in every one of the last thirty meltings but a single crystal was obtained. G. F. S.

NOTES FROM THE RESEARCH LABORATORY, EASTMAN KODAK COMPANY.*

THE EFFECT OF SCRATCHES ON THE STRENGTH OF MOTION PICTURE FILM SUPPORT.[1]

By S. E. Sheppard and S. S. Sweet.

THE general conclusion is that superficial scratches and a surface skin do not definitely affect the mechanical strength of motion picture film support. The effect of a scratch is determined by its depth affecting the thickness of the film. Except when near perforations, lateral scratches, less than one inch in width, do not appreciably affect the strength of the film support. Scratches made lengthwise the test piece have no effect on the strength.

THE ROTATING EARTH AS A REFERENCE SYSTEM FOR LIGHT PROPAGATION.[2]

By L. Silberstein.

THIS paper, read at the Lorentz Colloquium, University of Wisconsin, contains a systematic investigation of the properties of the rotating earth as a system of reference for optical phenomena and, more particularly, an exposition of the essence and meaning of the terrestrial rotational optical experiment undertaken at the author's instance by Professor Michelson and now (November, 1924) nearing completion. The result of the theoretical investigation is that on the relativity theory there should be a shift of the interference fringes by 1.38 fringe-widths per square kilometre of area embraced by the (triangular) optical circuit for a wave-length of 5000 Å., at the latitude of Chicago, and on the æther theory, a shift which, under the same circumstances, may have any value from zero to 1.38 according as the æther at the surface of the earth is sharing in its spinning motion completely or partially, or takes no part at all in the planet's rotation. Any

* Communicated by the Director.

[1] Communication No. 209 from the Research Laboratories of the Eastman Kodak Company and published in *Trans. Soc. Mot. Pict. Eng.*, No. 18: 102, 1924.

[2] Communication No. 212 from the Research Laboratories of the Eastman Kodak Company and published in *Phil. Mag.*, 48: 395, 1924.

shift smaller than 1.38 would definitely contradict the relativity theory, and would be compatible with an æther theory. The experiment is of interest as a possible crucial test. The results of the actual measurements, which are accurate to ±0.01 of a fringe-width, should be available before the end of the year.

NOTES ON THE PREPARATION OF STANDARD CELLULOSE. II.[3]

By A. B. Corey and H. LeB. Gray.

IN A previous paper it was noted that the standard cellulose, prepared by the tentative method described, gave too high an ash. It has been found that if the resulting cellulose, before drying, is treated with 1 per cent. acetic acid solution for two hours at room temperature and then washed with four changes of distilled water, the ash content is reduced to a very low quantity without any apparent injurious effect on the cellulose. In several samples the ash was reduced from 0.12 to 0.04 per cent. by this treatment.

The writers have observed, during the alkali boiling of different cottons, that with some a longer time is necessary to discharge the yellow color of the solution than with others. Results on one sample of cotton show that the percentage of α-cellulose may be slightly increased by boiling with the 1 per cent. sodium hydroxide solution for ten hours instead of six. It seems advisable to allow the displacing alkali solution to enter a little more rapidly so that twelve litres are used during the time of boiling; and also to continue the heating and displacement with 1 per cent. alkali for one hour after the yellow color has entirely disappeared.

THE PHOTOGRAPHIC REPRODUCTION OF TONE.[4]

(THE HURTER AND DRIFFIELD MEMORIAL LECTURE.)

By C. E. K. Mees.

NATURAL objects are seen as a series of tones differing in brightness, these differences being due to differences either in illumination or in reflecting power. The range of contrast varies from 1:2 to 1:1:100, most subjects falling between contrasts

[3] Communication No. 216 from the Research Laboratories of the Eastman Kodak Company and published in *Ind. Eng. Chem.*, 16: 1130, 1924.

[4] Communication No. 224 from the Research Laboratories of the Eastman Kodak Company and published in *Phot. J.*, 64: 311, 1924.

of 1 : 10 to 1 : 50. The reproduction of tones by photography can be followed by means of the graphic solution designed by L. A. Jones (Communication No. 88). A photographer must consider three variables in the process: (1) The contrast of the subject; (2) the contrast scale of the negative, γ; (3) the contrast of the printing paper. The contrast of the printing paper is a complex value involving its exposure scale, its maximum black, and its γ.

In printing the paper is selected by the rule that the density scale of the negative is equal to the effective exposure scale of the paper. As a result of a statistical inquiry, L. A. Jones has found that the limiting gradients of a paper depend upon the negative on which the paper is printed, greater limiting gradient being required for printing a flat negative than a contrasty negative. The limiting gradient multiplied by the γ of the negative is approximately a constant, this constant, however, depending on the density scale of the print.

The effective exposure scale of a paper is, therefore, dependent upon the γ of the negative and thus upon the contrast of the subject chosen. The contrast of a paper is, therefore, the density range of the paper multiplied by the slope of the characteristic curve of the paper integrated throughout its length with regard either to exposure or to density. The application of tone reproduction theory to various branches of photographic practice is discussed briefly.

IMPROVEMENTS IN MOTION PICTURE LABORATORY APPARATUS.[1]

By J. I. Crabtree and C. E. Ives.

A DESCRIPTION is given of the following motion picture laboratory apparatus: (1) An adjustable title-card holder permitting vertical and rotational movement of the title card by means of handles conveniently placed at the side of the camera, and adapted for making titles by reflected or by transmitted light. (2) A continuous film-viewing machine by which examination may be made of the continuity of negative or positive film for cutting out defects at the assembly table. (3) An improved semi-automatic sensitometer equipped with a timing device consisting of a make-

[1] Communication No. 206 from the Research Laboratories of the Eastman Kodak Company and published in *Trans. Soc. Mot. Pict. Eng.*, No. 18: 161, 1924.

and-break lamp circuit operated by a cam and sliding contact. (4) Two safety devices, (*a*) an electrical plug and socket so designed as to disconnect a metal machine; and (*b*) an inspection and assembly table with a "Vitrolite" top, permitting inspection by reflected light and equipped with a closed metal receiver for the waste film.

THE MAKING OF MOTION PICTURE TITLES.[8]

By J. I. Crabtree.

TITLES are classified and described as follows: (1) Uniform background titles; (2) illustrated background titles; (3) relief letter titles with plain or illustrated backgrounds; (4) scroll titles, and (5) animated titles. A complete description is given of the materials and apparatus required, including a camera and easel supported on a rigid track. Methods of preparing the reading matter, the size and nature of type for title cards, the requirements of the camera, the construction of a lens hood, the lighting equipment and the most suitable sensitive materials are described. Suggestions are included relative to the exposure and development of the film. The cause and prevention of troubles in title making are discussed.

THE PREPARATION OF THIOACETIC ACID.[9]

By H. T. Clarke and W. W. Hartman.

A NEW method for the preparation of thioacetic acid, in 70 per cent. yields, is by the action of hydrogen sulphide on acetic anhydride in the presence of a catalyst. Acetyl chloride, acetyl bromide, hydrochloric and hydrobromic acids are effective catalysts, acetyl bromide giving a more rapid reaction than acetyl chloride. The absorption of hydrogen sulphide, using the acid chlorides as catalysts, proceeds in a remarkable way. Twenty to thirty per cent. of the theoretical amount of hydrogen sulphide is absorbed in the presence of 0.15 per cent. of acetyl chloride or 0.22 per cent. acetyl bromide, while ten times this amount of catalyst is

[8] Communication No. 207 from the Research Laboratories of the Eastman Kodak Company and published in *Trans. Soc. Mot. Pict. Eng.*, No. 18: 223, 1924; *B. J. Phot.*, 71: 477, 1924; *Amer. Cinemat.*, 5: 9, 1924; *Sci. Ind. Phot.*, 4: 110, 1924.

[9] Communication No. 205 from the Research Laboratories of the Eastman Kodak Company and published in *J. Amer. Chem. Soc.*, 46: 1731, 1924.

necessary to effect absorption of 80 per cent. of the theoretical amount of hydrogen sulphide. Possibly the catalyst acts by inducing the formation of an active modification of acetic anhydride. Acetyl chloride alone, or acetic anhydride alone, does not react with hydrogen sulphide.

A MODIFICATION OF THE SANDMEYER SYNTHESIS OF NITRILES.[8]

By H. T. Clarke and R. R. Read.

THE ordinary Sandmeyer method of replacing aromatic amino groups by the nitrile radical is modified so as to avoid the formation of cyanogen or hydrocyanic acid. Instead of using acid solutions, the diazonium chloride solution is first neutralized with sodium carbonate, and is then added to a cold solution of cuprocyanide. The latter is prepared by adding sodium cyanide to cuprous chloride suspended in water. The yields of nitrile by this modified procedure are equal to those using the original Sandmeyer method.

Accurate Determinations of the Speed of Sound in Sea Water. E. A. ECKHARDT. (*Phys. Rev.*, Oct., 1924.)—On November 1, 1923, simultaneous radio signals and explosion waves in water were sent out by the U. S. Coast and Geodetic Survey vessel, *Guide*. Three and one-half pounds of TNT were exploded by the operation of a switch that in addition produced the radio signal. "It had been previously demonstrated by experiment that with the arrangement used the closing of the circuit, the detonation of the bomb, and the transmission of the radio signal are simultaneous to within .001 second." The position of the bomb was fixed with great care by surveying parties at two lighthouses about five miles from the ship. The wave of explosion travelled through water from 90 to 120 feet in depth over a distance of about sixty-one miles, 321,000 feet, to the receiving hydrophones. "The reception and recording of the signals were accomplished by the Subaqueous Sound-ranging Section of the Coast Artillery Corps at Fort H. G. Wright." "The time interval was of the order of 60 seconds and was measurable to the nearest .001 second. The end points of this distance were located on the map to within six feet."

One interesting feature of the series of observations is that they were all taken in a single afternoon. Three bombs were set off. Two hydrophones received the wave from the first, but four recorded the second and the third detonations. The speeds derived

[8] Communication No. 199 from the Research Laboratories of the Eastman Kodak Company and published in *J. Amer. Chem. Soc.*, 46: 1001, 1924.

from these two agree in a remarkable manner. Of the eight results the largest is 4896.21 feet, or 1492.37 metres per second, while the smallest is 4896.00 feet, or 1492.30 metres per second. Corresponding values from the first explosion are 4902.3 feet, or 1494.2 metres per second.

The intervals between the advent of the waves at the several hydrophones were used to calculate the speed of the sound over the distances separating these receiving instruments. For the second and third explosions the results obtained were 4895.1 feet per second and 1492.1 per second. The discrepancy between the values furnished by the first detonation, on the one hand, and the second and third, on the other, was felt to require an explanation. A partial one was found to lie in the fact "that at the time bomb No. 1 was fired there was a fairly strong tidal current running in the direction of sound propagation, while several hours later when bombs No. 2 and No. 3 were fired the tide was practically at a standstill."

Since there are now methods of measuring the depth of the ocean that depend on an accurate knowledge of the speed of sound in water, the practical value of the above measurements is obvious.

G. F. S.

Anomalies in Terrestrial Magnetism and in Gravitation Found in the Province of Kursk, Russia. P. LASAREFF. (*Rev. Gen. des Sciences*, Oct. 30, 1924.)—Were this part of Russia normal in respect to its terrestrial magnetism the lines connecting all points having equal horizontal components of the earth's magnetic field and also those lines connecting all points with equal vertical components would make only small angles with the geographical parallels of latitude. Instead of this being true, both of these lines are inclined to the parallels at angles greater than 45°. The area where the anomalies occur is about 22 km. by 30 or 40 km. in extent. The vertical component that would normally equal .4 or .5 C. G. S. in one place attains the value of 2 C. G. S. There are two localities within the region that show vertical components greater than 1.5 dynes. Owing to its great magnetic interest the magnetic elements have been observed at 14,000 stations within the province. There is in addition an anomaly of gravitation, but it does not coincide in its distribution with the anomaly of magnetism.

From a study of the unusual characteristics of the magnetism of the earth in this Russian province, it was concluded that the explanation was to be sought in the presence of magnetic deposits of ore. Borings have confirmed this. Deposits of magnetite have been found at a depth of 150 m. The mineral grows richer in iron as the depth increases, until it contains as much as 72 per cent. of iron in a mixture of hematite and magnetite. The implements used in boring became strongly magnetized.

It is calculated that from 5 to 33 billion tons of iron are contained in the deposit.

G. F. S.

NOTES FROM THE U. S. BUREAU OF CHEMISTRY.*

ABSORPTION AND RETENTION OF HYDROCYANIC ACID BY FUMIGATED FOOD PRODUCTS. PART II.[1]

By E. L. Griffin and E. A. Back.

[ABSTRACT.]

THE Bureau of Chemistry has continued its investigation of the absorption and retention of hydrocyanic acid by fumigated food products reported in Department Bulletin 1149.

All fruits which were not in tight packages absorbed some hydrocyanic acid. This was gradually dissipated, although a small quantity was left in many of the fruits at the end of ninety days.

Dried fruits fumigated in commercial packages did not absorb as much hydrocyanic acid as those fumigated without such protection. No fumigant was absorbed by apricots tightly packed in a box lined with waxed paper, and the gas did not penetrate to the centre of the box of tightly packed figs. This indicates that the process of fumigation used would not be effective as a method of freeing dried fruits from insects if the fruits are tightly packed and wrapped in waxed paper.

Candies absorbed a comparatively small quantity of hydrocyanic acid. The original absorption of nut meats was high, but most of the fumigant was dissipated in a very short time. Cocoa absorbed a high proportion of the fumigant and retained it for some time.

No hydrocyanic acid was absorbed through the rind of an American Swiss cheese. No fumigant penetrated to cheese tightly wrapped in tin foil, but the absorption was as high in cheese loosely wrapped in tin foil as in that which was not wrapped.

Boiling with water in the absence of added acid for thirty minutes was not always sufficient to entirely remove the hydrocyanic acid.

* Communicated by the Chief of the Bureau.

[1] Published as U. S. Dept. Agr. Bul. 1307, November 12, 1924.

THE SULPHURIC INDEX OF COTTONSEED OIL AND IN ADMIXTURES WITH OLIVE OIL.[2]

By James K. Morton and G. C. Spencer.

[ABSTRACT.]

THE principle advanced by Mazzaroni (*Staz. Sperim. Agrar. Ital.*, 48, 583–594 (1915)) has been utilized to identify cottonseed oil as well as to estimate the quantity of added olive oil. The arbitrary conditions here laid down must be closely followed. The method depends on the iodometric measurement of SO_2 liberated by unsaturated oil reacting with conc. H_2SO_4 (98.5 per cent.) under rigid temperature control. The extent of adulteration of cottonseed oil with olive oil may be determined within a definite limit, but the reverse is not true.

IS ABILITY TO UTILIZE CITRATE READILY ACQUIRED OR LOST BY THE COLON-AEROGENES GROUP?[3]

By Stewart A. Koser.

[ABSTRACT.]

EXPERIMENTS were conducted in the Bureau of Chemistry to determine whether intestinal *Bact. coli* cultures ever acquire the ability to attack citrate after a prolonged period in water or soil and whether *Bact. aerogenes* cultures ever lose their ability to utilize citrate when grown in an environment entirely different from that in which they were originally found and which simulates as nearly as possible the habitat of the intestinal *Bact. coli*.

The citrate differentiation, to separate the intestinal *Bact. coli* from other members of the colon group, appeared to be constant and reliable. The ability to utilize citrate is apparently a fairly stable character and evidently is not readily acquired or lost.

Domestic Production of Platinum in 1923.—According to JAMES M. HILL ("Mineral Resources of the United States," 1923, Part I, 9–22), during the year 1923, the production of crude placer platinum in the United States was 609 troy ounces, of which seven ounces were produced in Alaska, 578 ounces in California, and twenty-four ounces in Oregon. A total quantity of 3472 ounces of the platinum metals (platinum, palladium, iridium, osmiridium, and other metals of the group) were recovered in the refining of crude platinum, gold bullion, nickel, and copper of domestic origin.

J. S. H.

[2] Published in *J. Oil Fat Indust.*, 1 (Oct., 1924): 66.

[3] Published in *J. Infect. Dis.*, 35 (Oct., 1924): 315.

NOTES FROM THE BUREAU OF MINES, DEPARTMENT OF THE INTERIOR.*

FACTORS RETARDING TRANSMISSION OF RADIO SIGNALS UNDERGROUND.

By J. J. Jakosky and D. H. Zellers.

THE Bureau of Mines has been conducting experiments in underground communication during a period of several years: (1) With the geophone; (2) with short-wave radio equipment; (3) with ground conduction signalling; (4) an improved electrical geophone, and more recently (5) on the factors retarding transmission of radio signals underground.

There are four chief factors that affect electromagnetic field intensity, namely, (1) distance; (2) wave-length or frequency of carrier; (3) efficiency of transmitting antenna, and (4) attenuation. Of these, attenuation is of particular significance in underground radio communication. Attenuation is dependent upon two factors, namely, the decrease in current due to the geometrical spreading out of the electromagnetic waves in space; and to the absorption of waves in passing through the strata.

Three series of field experiments were conducted on (1) conductivity of exposed surface strata, and exposed underground strata in coal mines; (2) underground reception of radio signals, and (3) tests with small (low wave-length) transmitters at three wave-lengths, both above and underground.

The purpose of the earth-conductivity tests was to prove by actual experiment that different strata had varied resistance, in other words, that they affected differently the retardation of radio signals.

The results indicate that in general the earth resistance depends upon the percentage of moisture in the ground and the relative amount of soluble material contained in the strata. For example, coal has a comparatively low conductivity, while clay, which contains a relatively high percentage of moisture, has a high conductivity in the field, but a low conductivity in solution, due to the small amount of soluble materials. The surface soil, however, which is quite high in both moisture and soluble material, has a

* Communicated by the Director.

relatively high conductivity in the field as well as a high solution conductivity.

In the underground reception tests, it was found that the high absorption of the signals in penetrating the earth requires receiving equipment too sensitive for practical use in the mines.

The tests to determine the practicability of voice communication by pure radio signals, using small portable transmitters, indicate that for short distances (twenty-five to fifty feet) practically no difference is noted between surface and underground signal strength. At distances greater than this the losses in underground work cause a much higher attenuation of the transmitted energy.

Because of the high earth conductivity and resultant attenuation of the high-frequency radio waves in penetrating the earth, relatively high-power equipment, which means bulk and weight, is required. Such equipment is believed to be impractical for general mine communication or for mine-rescue purposes. A more detailed account will be found in Serial 2651 of the Bureau of Mines.

EFFECTS OF TEMPERATURE AND PRESSURE ON GYPSUM AND ANHYDRITE.

By Marie Farnsworth.

THE Bureau of Mines has recently undertaken a study of the chemical and physical properties of anhydrite having in view its wider utilization. Anhydrite ($CaSO_4$), in contact with water at ordinary temperature and pressure rehydrates, slowly forming gypsum ($CaSO_4 2H_2O$). In nature this is an extremely slow process, and laboratory experiments are being conducted to determine methods of accelerating the rehydration.

The method of testing the effect of high temperature and pressure on gypsum and anhydrite was as follows: The material was sealed with an equal volume of water in heavy pyrex test tubes. The tubes were then heated for forty-eight hours at temperatures varying from 100° to 210° C.; opened, the water filtered off, and the solid dried for several hours at 45° C., heated to constant weight in a platinum crucible at about 250° C.

The tests show that a pressure of nineteen atmospheres has no effect on anhydrite and water under high-temperature con-

ditions, and that anhydrite cannot be rehydrated under conditions of high temperature and pressure. Further details are given in Serial 2654.

THE ANALYSIS OF OIL-WAX MIXTURES.

By L. D. Wyant and L. G. Marsh.

A CONVENIENT method for the analysis of oil-wax mixtures has been developed in the petroleum laboratory of the Bureau of Mines. This method has been found useful in studying the characteristics of the products obtained in petroleum refineries during the manufacture of "wax distillate," "foots oil," "intermediate wax," and commercial paraffin. In this method acetone is used as a selective solvent.

The method was developed as part of the bureau's general study of methods of manufacturing lubricants from wax-bearing crude petroleums. In connection with this general problem, the characteristics of oil-wax mixtures have been studied, particularly the mixtures produced during the refining of "slack" wax to commercial paraffin.

Experience with the above method has shown that in general it gives reliable results if the melting point of the wax portion is normal. If the recovered wax has a low melting point, indicating an incomplete separation of oil and wax, it is redissolved with new acetone. If, on the other hand, the melting point of the wax is abnormally high, the percentage yield will probably be slightly low, and for accurate results it is necessary to recover an additional crop of wax crystals from the separated oil. This is done by redissolving in acetone, cooling to 60° F. and filtering. Further details are presented in a recent publication of the bureau (Serial 2655).

Separation of Gallium from Other Elements.—ERNEST H. SWIFT, of the California Institute of Technology (*Jour. Am. Chem. Soc.*, 1924, **46**, 2375–2381), finds that gallium trichloride is extracted almost completely by ethyl ether from its solution in 5 normal to 6 normal hydrochloric acid; the maximum extraction by the ether occurs when the hydrochloric acid has a concentration approximately 5.5 normal. Gallium may readily be separated from iron by means of an excess of sodium hydroxide, which precipitates the iron as ferric hydroxide while the gallium remains in solution. The sodium hydroxide must be present in such an excess that its concentration is not less than 0.3 normal. J. S. H.

A Direct Proof That the Earth Has an Electrical Charge. C. RAMSAUER. (*Ann. d. Phys.*, Vol. 75, No. 21, 1924.)—From the rate at which the electrical potential decreases along a line drawn vertically upward from the earth, it is possible to derive the charge of the earth and also its potential. For a decrease of 100 volts for one metre of ascent the total charge of the earth would be 500,000 coulombs of negative electricity and the potential of our planet must accordingly be –600,000,000 volts. Such being the conclusions to which theory leads, the author proceeds to devise a method of measuring directly the electrical charge on a definite area of the earth's surface. In addition to measuring instruments and contact key, the apparatus consists essentially of three simple conductors, (*a*) a horizontal plate of zinc, 1 m. square, permanently connected to earth; (*b*) a second zinc plate, 90 cm. square, supported above the first plate and parallel to it by amber insulators, and (*c*) a metal cover that sets over the second plate without touching it and rests with its projecting edges on the first. In order to get larger charges, the apparatus was set up on the roof of the chemical laboratory of the Technical College at Danzig.

With the cover removed the second plate was connected to earth, thus becoming a part of the outer surface of the earth and receiving a part of the earth's electric charge. After connection to the earth was broken, the cover was put over the second plate, which still retained its charge, though no longer a part of the earth's surface. This plate was then discharged through a galvanometer, and the connection to earth removed. Upon the removal of the cover the second plate again resumed its place as a part of the surface of the earth, though with no charge. When it was joined to earth a charge flowed to it that was measured by the galvanometer. One millimetre of deflection of the ballistic galvanometer corresponded to the passage of 15×10^{-10} coulombs of electricity. The currents at charge and discharge mentioned above produced deflections never exceeding 5 mm. From the mean value of the deflections the charge on the building per square metre is calculated to be 64×10^{-10} coulombs, or the same area has 4×10^{10} more electrons than it would have were it neutral electrically. The electrometer was used to confirm the results obtained with the galvanometer.

On the earth itself the charges were only about 40 per cent. as great as on the building. The highest value found for the level earth was a charge of 15×10^{-10} coulombs per sq. m. This means the presence on this area of 1×10^{10} electrons. The variation of the charge with time was considerable. In one case at the beginning of a light rain this sank in a few minutes from 14×10^{-10} coulombs to zero. In 10^{-10} coulombs per sq. m. the maximum charges observed were as follows: On the roof of the college, 125; on the level earth, 15; at 5 m. from the building 1.5. G. F. S.

THE FRANKLIN INSTITUTE.

(Proceedings of the Stated Meeting held Wednesday, December 17, 1924.)

HALL OF THE FRANKLIN INSTITUTE,

PHILADELPHIA, December 17, 1924.

PRESIDENT DR. WM. C. L. EGLIN *in the Chair.*

Additions to membership since last report, 4.

The following nominations were made for officers and members of the Board of Managers to be voted for at the annual election to be held on Wednesday, January 21, 1925:

FOR PRESIDENT (to serve one year), Wm. C. L. Eglin.

FOR VICE-PRESIDENT (to serve three years), Henry Howson.

FOR TREASURER (to serve one year), Benjamin Franklin.

FOR MANAGERS (to serve three years), Francis T. Chambers, Alfred C. Harrison, Nathan Hayward, Charles A. Hexamer, Robert W. Lesley, Marshall S. Morgan, E. H. Sanborn and J. T. Wallis.

The next business in order was the consideration of the proposed revised By-Laws. The Secretary called attention to the changes which have been made in the printed copy circulated to members about two months ago. These changes were made after correspondence with members who had given the matter due consideration and offered suggestions. On motion, duly seconded, the revised By-Laws were unanimously adopted.* The following preamble and resolutions were then offered:

"Since the adoption of the new By-Laws terminates the office of the Board of Trustees existing under the former By-Laws and places the administration of the assets and securities of the Institute at the disposal of a Committee on Finance of the Board of Managers, the following resolutions are offered for consideration:

"*Resolved,* That the previously existing By-Laws be, and are hereby repealed.

"*Resolved,* That the Board of Trustees be requested, authorized and directed to assign, transfer and deliver all of the assets and securities of the Institute held by the Board of Trustees to the Committee on Finance of the Board of Managers and that upon making such transfer and deliveries the Board of Trustees be discharged and relieved of all responsibility and liability in respect thereto, and that the Board of Managers be authorized and empowered to carry out this resolution."

These, being duly seconded, were unanimously carried.

The Chairman announced that the next order of business would be the presentation of the Howard N. Potts Medal, and recognized Dr. Winthrop Wright who, on behalf of the Committee on Science and the Arts, introduced

* The By-Laws as revised will be published in the next issue of the Year Book.

Dr. John A. Anderson of the Mount Wilson Solar Observatory, California, to whom had been awarded the Howard N. Potts Medal for his improvements in the ruling of gratings and in the invention of a new form of seismograph. The Chairman presented the medal and accompanying documents to Doctor Anderson, who expressed his appreciation of the honor conferred upon him.

The paper of the evening on "Low Temperature Coal Carbonization and Its Prospective Industrial Application" was then presented by Dr. Horace C. Porter, of Philadelphia. The speaker pointed out that low temperature carbonization offers many features of promise, with a view to relief of the increasing shortage of clean, solid fuel for domestic use, mitigation of the smoke nuisance from bituminous coal, and the making of valuable by-products. He called attention to some serious practical difficulties in the method of operation. Four processes, now under active industrial experimentation in America, were described. The subject was illustrated by experimental demonstrations, samples of commercial products and lantern slides. After a brief discussion the thanks of the meeting were extended to Doctor Porter for his interesting paper.

Adjourned.

GEORGE A. HOADLEY,
Acting Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

(Abstract of Proceedings of Stated Meeting held Wednesday, December 3, 1924.)

HALL OF THE INSTITUTE,
PHILADELPHIA, December 3, 1924.

MR. GEORGE H. BENZON, JR., *in the Chair.*

The following report was presented for final action:

No. 2791: Nachod Electric Railway Signal System.

The Nachod Electric Railway System of Signals is adapted to the control of both city and suburban service in which the overhead trolley is employed. A form of contactor is used with which no disturbance of the passing trolley is necessary, since no part of the apparatus is associated with the tracks. The car operating current is used for signals, both for the relay operation and for the light signal, and no shock is communicated to the overhead wire structure by the passage of a car, even at high speeds. This is brought about by the use of a contactor consisting of two insulated metallic strips parallel to the track, to which the signal relay is connected and between which the trolley wire passes.

When the trolley wheel passes it bridges the gap between the wire and one of the strips, allowing current to pass from the trolley wire to the signal. This is independent of the position of the motorman's controller, whether the car is running under power or coasting. The use of this form of contactor is one of the distinguishing features of this system and the device is covered by several United States patents issued to Mr. Nachod.

In consideration of the ingenuity and novelty of design and of general excellence of construction, The Franklin Institute awards its Edward Longstreth Medal to Carl P. Nachod, Esq., of Louisville, Ky.

GEORGE A. HOADLEY,
Acting Secretary.

MEMBERSHIP NOTES.

ELECTIONS TO MEMBERSHIP.

(Stated Meeting, Board of Managers, December 10, 1924.)

RESIDENT MEMBERS.

MR. E. C. B. KIRSOPP, Executive, Rohm and Haas Company, Inc., Manufacturers of Chemicals, 40 North Front Street, Philadelphia, Penna.

NON-RESIDENT MEMBERS.

MR. J. HENRY HALLBERG, Electrical Expert, 445 Riverside Drive, New York City, N. Y.

MR. FRANCIS T. HOMER, Lawyer, Riderwood, Md.

ASSOCIATE MEMBERS.

MR. KENNETH M. RITCHIE, Chemist, in care of Armstrong Cork Company, Gloucester City, N. J.

CHANGES OF ADDRESS.

MR. WILLIAM H. BALLS, South Philadelphia High School, Broad and Jackson Streets, Philadelphia, Penna.

MR. F. M. BENNETT, 2507 Master Street, Philadelphia, Penna.

DR. JOHN PRICE JACKSON, Room 702, New York Edison Company, Irving Place and 15th Street, New York City, N. Y.

MR. ELISHA LEE, Vice-president in charge of Operation, Broad Street Station, Philadelphia, Penna.

DR. C. E. K. MEES, Kodak Park Works, Rochester, N. Y.

MR. C. B. NEBLETTE, Department of Photography, Texas A. and M. College, College Station, Texas.

MR. T. R. PARRISH, 1036 Randolph Street, Oak Park, Ill.

MR. FRANCIS DU PONT THOMSON, 241 E. Main Street, Elkton, Md.

MR. WM. S. TWINING, 127 Park Avenue, Fort Myers, Fla.

MR. FRANCIS R. WADLEIGH, 1 Broadway, New York City, N. Y.

MR. G. A. WELLS, 74 S. Munn Avenue, East Orange, N. J.

MR. CHARLES N. WEYL, 255 Harvey Street, Germantown, Philadelphia, Penna.

NECROLOGY.

Mr. Franklin M. Harris, Belle Claire Apartments, 40th Street and Parkside Avenue, Philadelphia, Penna.

Mr. C. Stuart Patterson, Western Savings Fund Society of Philadelphia, 1004 Walnut Street, Philadelphia, Penna.

Mr. W. H. Sharp, The Wellington, 19th and Walnut Streets, Philadelphia, Penna.

RESIGNATION OF R. B. OWENS, D.S.O., D.Sc., F.R.S.C., OF THE OFFICE OF SECRETARY OF THE FRANKLIN INSTITUTE AND OF THE DIRECTORSHIP OF THE BARTOL RESEARCH FOUNDATION.

The Board of Managers records the resignation of Dr. R. B. Owens, for fourteen years Secretary of The Franklin Institute and Editor-in-Chief of its JOURNAL, and for the past three years Director of the Bartol Research Foundation. Doctor Owens' term of office as Secretary and Editor has been coincident with a period of Institute activities and achievements worthy, we believe, of a place among the brighter epochs of its one hundred years of useful life. Doctor Owens brought to the work of the Institute not only a trained mind and a notable store of scientific knowledge, but also a remarkably extensive acquaintance with men whose lives, devoted to work in the field of physical science, have been conspicuous in service to mankind.

Speaking for the management of the Institute, we wish him an early and complete restoration of the health which has lately been seriously impaired, and a long life of enjoyment in the pursuit of his beloved science and in the fellowship of his many friends in the scientific world.

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BOOK REVIEWS.

INTRODUCTION TO ORGANIC RESEARCH. By E. Emmet Reid, Johns Hopkins University. viii–330 pages and indexes, 8vo. New York, D. Van Nostrand Company, 1924. Price, $5.

Of late years much effort has been made both from the platform and by literature to encourage research in this country. The urge has been largely from a consciousness of the value of it to the safety of a nation. The German efficiency as manifested in the war was a lesson to all other nations. It is particularly in organic chemistry that German work was pushed so thoroughly. Even many years ago a German writer could say with truth that the coöperation of theory and practice had enabled the coal-tar color industry of his country to conquer the world, and while he and all his readers understood this as referring to industrial conquest, the events of the war demonstrated that the Teuton came very near a conquest of an entirely different kind.

The people of the United States cannot be accused of indifference to the value of investigation, nor of neglect of it, for a long list of inventions and discoveries is to their credit. These, however, have been very largely in the mechanical line. Improvements in agricultural machinery, in the power-printing press, in apparatus for the transmission of intelligence, in the airplane, submarine and motor car, and in a great number of minor constructions designed to save labor, especially in the household and business office, in which connection we should not forget the vacuum cleaner, typewriter and cash register.

Much propaganda has been inaugurated concerning research in organic chemistry and an immense field, which notwithstanding the amount of work that has been done in it, especially by Germans, still offers opportunities in many ways. The work in hand is intended to assist in this line and to encourage young chemists to enter upon it. It is true, of course, that no book can take the place of the actual service of the teacher, but a carefully written treatise, by one who has had experience in carrying out as well as directing research will be of much value, and such is the volume in hand.

In the preface, Professor Reid tells us that Tubal Cain was not only a metallurgist, but a research worker as far as he sought to improve his methods of producing iron. In the latter statement he seems to go beyond the text, but the matter is not important. In both the preface and the somewhat extensive introduction, the author allows his spirit free range and thus aids in infusing some of his enthusiasm into the reader and user of the book. The book is not a mere manual of chemical methods nor a collection of suggestions for investigation. Over fifty pages are devoted to a more or less philosophic and

academic discussion of the value and installations for research, after which chemical literature is taken up. In connection with this subject, the reviewer is very glad to see that a knowledge of French and German is advised strongly for the English-speaking worker. The discussion on this subject, as given on pages 65–67, should be read and remembered by every professor in the school in which advanced instruction in chemistry is given. In the vast majority of cases, while the catalogue of the institution may have French and German as requirements and the respective teachers of those languages may labor earnestly to equip their pupils with a good reading knowledge thereof, close inspection will show that most students get but a smattering, look on the studies as more or less of bores, and after securing the certificate or diploma, allow what little knowledge they have to die out and content themselves with abstracts published in their native tongue. It is undoubtedly true that British and American abstract service in chemistry is very efficient, but it is never safe to rely entirely on second-hand information, and the reviewer's long experience and extended opportunities for observing the reactions of students to studies of French and German, have led him to the view that much more severe requirements in these respects should be enforced, and that no student should receive a degree in chemistry who has not a good reading knowledge of French and German scientific literature.

Professor Reid's book will serve a good part in a line of development in American chemistry that is much needed. HENRY LEFFMANN.

QUANTITATIVE ORGANIC MICROANALYSIS. By Fritz Pregl, D.Sc., Ph.D., University of Graz. Translated from the second revised and enlarged German edition by Ernest Fyleman, B.Sc., Ph.D., F.I.C. xv–186 pages, index and 42 illustrations. Philadelphia, P. Blakiston's Son and Company, 1924. Price, $4.

This work will open up to physiologic and pharmacologic chemists many interesting and useful procedures. Microanalysis has, indeed, been extensively cultivated for many years, and has perhaps found its most extensive development in inorganic, especially in mineralogic chemistry. The specific subject of this book is minute analysis with gravimetric determinations. The text begins with a full description of the delicate balance to be employed. All manipulations and procedures are described in great detail and numerous illustrations aid materially in the explanations. In the inorganic field, the microscope has found extensive application in dealing with minute amounts and the new methods of examination by determination of refractive index enable the mineralogist to identify positively many rock constituents.

As the translator remarks, a characteristic feature of modern research is the attention given to the presence of compounds in minute amounts. He refers only to organic research, but the same is true in the inorganic field. The detection of radium as the main cause of the peculiar actions of uranium compounds is a case in point. In organic chemistry an early instance of the value of recognizing an ingredient present in small amount was in the test for benzene, originally thought to be indicative of the hydrocarbon itself, but later discovered to be due to a sulphur compound.

The translation is in the main well done, but now and then German idioms appear. These are not misleading but they are sometimes a little puzzling. Taken all in all, however, the book is a valuable introduction to a field of wide importance and interest. HENRY LEFFMANN.

THE RECENT DEVELOPMENT OF PHYSICAL SCIENCE. By William Cecil Dampier Whetham, M.A., F.R.S., Trinity College, Cambridge. Fifth edition, 1924, xvi–306 pages, index, 42 illustrations, mostly plates, small 8vo. Philadelphia, P. Blakiston's Son and Company. Price, $3.

The first edition of this book appeared in 1904; second and third editions appeared before the close of the year, and the fourth appeared in 1909. In the preparation of the present issue the author has had, of course, to rewrite the book in great part, for great advances have been made in all departments of physics in the fifteen years that have elapsed. While not primarily intended as a textbook, information is furnished couched in language intelligible to the well-educated student. A wide scope of subject-matter has been chosen, since astrophysics and discussion of the recent developments of relativity are included. Numerous illustrations, many of them photogravures, are included and add to the usefulness of the work. The author is not without "feeling in his business," for he gives to each chapter a quotation expressing some phase of the subject in hand. It would, however, be much more satisfactory if the Latin phrases had been translated. British scientists in part may be good classicists, but there has been a falling off in this direction even among them, and American scientists as a mass have "small Latin and less Greek." Bacon is quoted in the book several times. There are satisfactory translations of his writings and these should have been used. Some slight justification might be found for retaining the original language of Lucretius for that was his tongue, but Bacon's Latin was an artificial construction, appropriate in his day to be sure, but not adapted to modern use.

The book contains a large amount of information well arranged and will aid those who wish a summary of the important features of progress in pure science in the field of physics during recent years. It is quite up to date in the data furnished. HENRY LEFFMANN.

THE PRINCIPLES OF ELECTROCHEMISTRY. By H. Jermain Creighton, Swarthmore College. In two volumes, Vol. I, ix–424 pages and index, 76 illustrations, 120 tables, 8vo. New York, John Wiley and Sons, Incorporated, 1924. Price, $4.

Electrochemistry has grown to be of such practical and theoretical importance, that it now occupies the exclusive attention of a large number of specialists. The organization and activity of the American Electrochemical Society is a striking evidence of this. The beginnings of the subject as an exact science are within the memory of many chemists now living. It is not usually possible to say of any development in science when the first step was made or who made it, but the work of Faraday was certainly essentially initiative in this field. While an immense amount of literature, both as to principles and practical results, has been published of late years, no book has appeared in English dealing with the

subject. The work will be in two volumes. For the volume in hand Doctor Creighton is responsible, but as the name of Dr. Colin G. Fink is associated in the announcement of the full title which the complete work will bear, namely "Principles and Applications of Electrochemistry," it is to be presumed that the latter will be largely concerned in the second volume. The two volumes will constitute a valuable addition to the literature of a difficult and very important subject, and the authors and publishers are to be commended for undertaking the duty of preparing and issuing the work.

In method the text presents a systematic course of instruction in electrochemistry for students, who, however, are presumed to be familiar with the elementary principles of physical chemistry and physics. We are informed in the advertisement of the book that the title is broadly interpreted, as the work will include both electrometallurgy and electrothermics. Keeping in view the use of the book by students, problems have been given at the end of nearly every chapter. The references to the literature are extensive and detailed. It is gratifying to note that both number and volume are given. One feature, however, of the bibliography does not meet with the reviewer's approval. This is the use of *Loc. cit.* It is best to repeat the reference in full or at least attach some brief identification term.

The last chapter is devoted to the electrochemistry of gases. This, as the author informs us, is an entirely new subject, and it is believed that no other book contains any comprehensive data on the topic in the extended form that it appears in this book. Specialists in this field have highly commended the treatment given.

The English-speaking student who desires a knowledge of the present development of the important field to which the book is devoted will find great assistance in its pages, and the practical chemist will be equally aided in solutions of the problems with which he is confronted. It is to a certain extent a disappointing situation to the older group of chemists to note how steadily their science, at least so far as general principles are concerned, is being submerged in physics. A large proportion of the chemical works of the present time seem on reading to be more physical than chemical.

The mechanical execution of the book is excellent.

HENRY LEFFMANN.

PUBLICATIONS RECEIVED.

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A Systematic Handbook of Volumetric Analysis on the Quantitative Determination of Chemical Substances by Measure, Applied to Liquids, Solids and Gases, by Francis Sutton, F.I.C., F.C.S. Eleventh edition revised throughout, with numerous additions, by W. Lincolne Sutton, F.I.C., and Alfred E. Johnson, B.Sc., Lond., F.I.C. 629 pages, illustrations, 8vo. Philadelphia, P. Blakiston's Son and Company, 1924. Price, $9.

Materia Photographica. A dictionary of the chemicals, raw materials, developing agents and dyes used in photography, by Alfred B. Hitchins, Ph.D.

95 pages, 12mo. Philadelphia, Frank V. Chambers, 1924. Price, in paper, 50 cents; in cloth, $1.

Dynamo Electric Machinery. The theory, construction and operation of direct and alternating current machines, by Erich Hausmann, E.E., Sc.D. 645 pages, illustrations, 8vo. New York, D. Van Nostrand Company, 1924. Price, $4.50.

National Advisory Committee for Aeronautics. Technical Notes, No. 208, Tests on Duralmin Columns for Aircraft Construction, by John G. Lee. 4 pages, illustrations, quarto. Washington, Committee, 1924.

An Electric Arc without an Intensely Hot Cathode. HELGE STOLT, Upsala. (*Zeit. f. Physik,* Vol. 26, No. 2, 1924.)—It is customary to base explanations of the electric arc on the high temperature of the cathode, and there is experimental evidence from no less an authority than J. A. Fleming in support of the view that a white-hot cathode is necessary for the existence of an arc. On the other hand, Nicol in 1909 succeeded in making an arc rotate between copper electrodes in the form of rings, which remained cool. In the present paper this effect was obtained with gold and silver as well as with copper electrodes. To get the speed of rotation of the arc the electrodes were photographed, but 1/500 second after the arc had passed, the parts of the electrodes where it had been were not hot enough to produce a photographic impression. When the current strength was about 10 amperes the electrodes were too hot for the finger to touch them permanently, the estimated temperature being 200° C. For low-current strengths at which the arc formed, the finger could actually rest on the electrodes without discomfort immediately after the arc stopped.

The Swedish investigator further maintained an arc between a fixed anode and a rotating cathode of copper, gold or silver. The first of these gave the best results. The arc appeared to be dragged along by the rotating cathode so that its length made an acute angle with the latter. When the current was kept constant an increase in the velocity of rotation required an increase in the voltage applied. Here again the test of the applied finger found no evidence of high temperature on the cathode. The author concludes: "My experiments show with great probability that the electrons in the arc are released without a high temperature in the cathode." G. F. S.

Cornstalk Syrup.—The manufacture of syrup from cornstalks has been studied by J. J. WILLAMAN, G. O. BURR, and F. R. DAVISON, of the University of Minnesota; the results of their research are reported in Bulletin 207 of the Agricultural Experiment Station of that University (58 pages, March, 1924). The yield of stalks per acre is three or four tons with the smaller varieties of sweet corn, and eight to ten tons with the larger varieties of sweet corn and with field corn. The yield of syrup per ton of whole cane is as low as

eight or nine gallons if the stalks be used immediately after removing the ears, and is ten to twelve gallons if the stalks be allowed to stand for two or three weeks after removal of the ears. The cornstalk syrup is usually clear, and has a reddish amber color and a mild agreeable flavor. It is not a table syrup, but a cooking syrup, with characteristics and uses very similar to those of sorghum and molasses. The process of manufacture is essentially the same as for sorghum syrup: Controlled defecation, filtration, and vacuum evaporation. This syrup may become a by-product of corn canneries. J. S. H.

Pharmacopœial Standards for Whisky and Brandy.—While both whisky (*Spiritus frumenti*) and brandy (*Spiritus vini Gallici*) were official drugs in the eighth decennial revision of the "United States Pharmacopœia" (1900), they were omitted from the ninth decennial revision in 1910. In view of the widespread adulteration of the alcoholic liquors at present available for therapeutic use, the Committee on Revision has decided to include both whisky and brandy in the tenth decennial revision of the Pharmacopœia, and to set standards of purity for both liquors when used as medicines. These standards then become the legal standards under the Federal Food and Drugs Act. (*Jour. Am. Pharmaceutical Asso.*, 1924, **13**, 159). The proposed sections on whisky and brandy, which is now designated *Spiritus vini vitis* (*Jour. Am. Pharmaceutical Asso.*, 1924, **13**, 1058–1060), give the per cent. of alcohol by volume at 15.56° C. Whisky must contain not less than 47 and not more than 53 per cent. of alcohol, brandy not less than 48 and not more than 54 per cent. of alcohol. Both whisky and brandy must have been stored in wood containers for at least four years. In addition to other standards, tests are given for the detection of acetone, methanol, alkaloids, diethyl phthalate, formaldehyle, heavy metals, isopropyl alcohol, and phenols in brandy, and for the detection of the compounds just enumerated, caramel, glycerol, and sugar in whisky. These compounds include the common denaturants and adulterants. J. S. H.

War Gases for Insect Control. (U. S. Department of Agriculture, Clip Sheet No. 337, December 8, 1924.)—In coöperation with the Chemical Warfare Service, work is being carried on by the Bureau of Entomology of the United States Department of Agriculture with the so-called war gases and other materials developed at the Edgewood (Md.) arsenal. Many of these substances have been tested for the purpose of determining their availability in insect-control work and for other practical uses. A few have been found which give promise of value. A smoke candle for use in greenhouses has been developed and is being tested on a commercial scale. It seems to have a distinct place in greenhouse insect-control work.

CURRENT TOPICS.

The Penetrating Radiation in the Higher Atmosphere. A. WIGAND. (*Physik. Zeit.*, Sept. 15, 1924.)—In this article is given a résumé of the present state of knowledge of this subject. It is believed that there is in the atmosphere a type of radiation of such penetrative power as to warrant the extension of the wave-lengths of the electromagnetic spectrum beyond the shortest of the gamma rays, unless, indeed, the effect be finally attributed to streams of corpuscles. This radiation is of importance on account of its presumed connection with physical phenomena on the sun, with terrestrial magnetism and the aurora and with the problem of the maintenance of the electrostatic field of the earth.

In 1909 Gockel found by observations made in the flight of a free balloon that the radiation that entered through the metallic walls of a closed chamber did not diminish in intensity at an elevation of 4.5 km. as he would have expected it to do, had it consisted of γ-rays originating in the earth. Further balloon experiments confirmed this observation and contributed the additional fact that as the balloon ascends there is first a decrease in the radiation up to a height of perhaps 2 km., after which there comes a slow increase. Other investigators examined the phenomena and in the main corroborated Gockel's observations. The following data from Kohlhoerster give an idea of the magnitude of the effects, after subtraction of the effect of the radiation found at the level of the ground. At 2 km. above sea level the radiation produces in a second one pair of ions in a cubic centimetre of air. Corresponding numbers for elevations of 3, 4, 5, 6, 7, 8, 9 and 9.3 km. are 4.2, 9.1, 16.2, 28.0, 45.2, 62.5, 79.0, and 85.0 pairs of ions. Millikan and Bowen sent pilot balloons up to 15 km. and got increases much smaller than those derived from extrapolation of Kohlhoerster's curve. There appears to be some change from time to time in the intensity of the penetrating radiation but very little is known about this. There seems to be about the same intensity during the day as at night and an eclipse of the sun is without effect. There was, however, a genuine increase in the radiation in May, 1910, at the time of Halley's comet.

The source of the penetrating radiation has been located either outside of the earth or in the higher regions of the atmosphere, but no hypothesis has vanquished its competitors. The sun may send to the earth streams of α- or of β-particles. These impinging on molecules of gas or on dust particles in the atmosphere of the earth may give rise to the penetrating radiation. Or, again, certain regions of the sky where there are stars of small density and young red stars may, according to Nernst, send the radiation to the earth. Still another hypothesis is that an accumulation of radio-active material

exists at the boundary separating the troposphere from the stratosphere of the earth, that is at about 11 km. above the earth, or, perhaps, in the lower stratosphere. More accurate observations are needed in order to form a basis for the solution of the many problems concerning this radiation that as yet have no solution. Of these, one of the most important is to find whether, as is generally assumed, the ionizing effect is due to a type of radiation or, on the contrary, to a stream of corpuscles. G. F. S.

A New Method of Determining the Dew Point. M. HOLTZMANN. (*Phys. Zeit.*, Sept. 15, 1924.)—The surface on which the deposit of the layer of water is to be observed is that of a copper tube 15 cm. long and 2 cm. internal diameter. A liquid at a temperature below the dew point enters the tube at its lower end and leaves at the top. Around the upper part of the tube is wrapped a coil of wire through which a current of electricity flows. Thus somewhere between the coil and the lower end of the tube the metal of the tube is at the temperature of the dew point. The outer surface of the tube is polished. Perpendicular to the vertical axis of the tube is a line on the outer surface of the tube. To this are soldered at some distance apart the ends of a thermocouple. By manipulating the flow of cool liquid and the strength of the electric current the line of demarcation between the portion of the tube covered with condensed water and that part dry because its temperature is above the dew point is caused to travel along the tube until it coincides with the line. The temperature of the metal under the line is then found from the reading of the galvanometer in series with the thermocouple. After practice the observer gets readings of the dew point that differ by only .02 or .03 degree.

This communication comes from the Laboratory for Experimental Geophysics at Leningrad. G. F. S.

The Radio-activity of Gases from Vesuvius and from Solfataras in the Campania. J. STOKLASA and J. PENKAVA. (*Comptes Rendus*, Oct. 27, 1924.)—At the foot of the Eiffel Tower the electric conductivity of the air was found to be $.4 \times 10^{-4}$ electrostatic units, while at the top it was twice as great. It was impossible to get gas at the vent of Vesuvius, but after it had mingled with air its conductivity was 4.8×10^{-4}. At Pozzuoli and Posillipo, near regions of heated earth and fumaroles, the air had a conductivity of about 2.5×10^{-4}. In Jachymov in Joachimstal, near the radium factory, the value is 2.4×10^{-4}, while the gas coming from Forum Vulcani near Naples possesses a conductivity of from 2.8 to 3.6×10^{-4}. Since potassium is radio-active it was desired to examine the air at the potassium deposits in Alsace at Mulhausen. There over the NaCl the conductivity was $.4 \times 10^{-4}$, but in the strata of KCl it rose to 2.8, and at the heaps of this salt removed from the mine to 3.6×10^{-4}. G. F. S.

Acceleration of the Decay of Light from Phosphorescing Bodies. E. RUPP. (*Ann. d. Physik*, Vol. 75, No. 3.)—For a long time it has been known that heating a phosphorescing substance makes its light die away more rapidly and that the same effect is caused by illuminating it with light different from that which excited it. More recently Gudden and Pohl found that the application of a strong electric field brings about the same result. In this paper it is shown that a strong magnetic field also is effective. No effect is produced by magnetic fields of strength less than 20,000 gausses. Even with the highest attainable strength, 45,000 gausses, the change was much less than that due to the application of a field of 10,000 volts per cm. The sudden creation of the magnetic field causes the phosphorescing substance to grow brighter, after which there comes a period of decay. Moreover the light emitted by ZnS when the field is turned on is partially polarized perpendicular to the magnetic lines of force.

G. F. S.

Gravitational Anisotropy of Crystals. PAUL R. HEYL. (Scientific Papers, Bureau of Standards, 482.)—"Einstein's theory of gravitation is based upon a fundamental postulate which asserts that gravitation and inertia are identical in nature and hence indistinguishable. This is, if true, of the greatest theoretical importance, for gravitation has hitherto refused to show any relationship to other physical phenomena. A most delicate test of this postulate is possible in a crystal of one of the non-isotropic systems; for in such a crystal every known physical property (except inertia and, possibly, weight) varies with the axial direction in the crystal; and it is an interesting question whether, in such a crystal, gravitation will be found to align itself with inertia or will show some variability that will classify it with the great majority of physical phenomena." Doctor Heyl weighed large crystals of all the five non-isotropic systems, placing them in different axial positions. Moreover he weighed them to one part in 10^9, but he could find no difference in the weights, so that his results declare in favor of Einstein's postulate. G. F. S.

An Experimental Study of the Viscous Properties of Water Vapor. C. J. SMITH. (*Proc. Royal Soc.*, A 735.)—Though steam is of so great technical importance, but little attention has been paid to its viscosity. In this paper this property has been investigated at temperatures ranging from 100° to 260° C. Pressures of only a few centimetres of mercury were used, but, since "the work produces reliable evidence that under the experimental conditions the viscosity is independent of pressure," the results attained can be applied to the higher pressures that obtain in practice. For the four temperatures, 100°, 151.2°, 207.1° and 261.3° C., the coefficient of viscosity is found to equal 1.27, 1.45, 1.68 and 1.90, all multiplied by 10^{-4}.

G. F. S.

Copper Which Bends Only One Way.—Copper bars that can be bent double with one finger, but which require strength to straighten again, are expected to lead to a greater understanding of the properties of metals. The bars, which are really single crystals of pure copper, were produced in the Research Laboratory of the General Electric Company, at Schenectady, N. Y., and have been subjected to many kinds of examinations, with the revelation of numerous unexpected facts.

Knowledge about the properties of metals has been limited in the past to observations of masses of small crystals. The usual piece of metal is a conglomeration of small, closely packed crystals, with the crystalline structure usually apparent at a glance. Zinc, for instance, is known as a brittle metal; a rod of it can be bent but slightly without snapping. Yet investigations of small, single zinc crystals show that any one crystal of the metal can be drawn out to six times its length in one direction; in another direction it is extremely brittle. The properties of zinc thus depend upon how the crystal is examined—whether " with the grain " or against it. The usual piece of zinc is really a collection of small crystals pointing in all directions, so that the properties are the combined qualities of the small crystals in the different axial directions. The same holds true for other metals and other substances.

A single crystal of copper seven-eighths of an inch in diameter and six inches long, as well as numerous smaller crystals of the same metal, have been produced by Dr. Wheeler P. Davey, of the Research Laboratory. These crystals, obtained by a modification of the method devised by Dr. P. W. Bridgman, of Harvard University, are much larger than any previously recorded.

Very gradual heating and cooling of pure copper in an electric furnace is the secret of the success in producing them. The necessary amount of pure copper, in the form of a bar, was placed in a closed, cylindrical carbon crucible, and slowly passed through the electric furnace. If molten metal is cooled quickly, the resultant mass is composed of very small crystals; if the melt is cooled slowly, the crystals are larger. Doctor Davey cooled the melt so slowly that only one crystal was produced, and that included the entire melt. The atoms had plenty of time in which to arrange themselves as they desired—to build up a single crystal rather than a multitude of small ones.

Several interesting results have been obtained with the large crystals. A piece about the size of a lead pencil, if given a jerking motion, bends as easily as does a stick of soft wax; it cannot be bent back, however, any more easily than a similar piece of ordinary copper. When the copper is a single crystal, all of the atoms are arranged in columns, equally spaced. When the bar is bent, the spacing is changed; the atoms on the inside curve are pressed together, and those on the outside are spread apart. Strains are set up and the

crystal structure is altered. The bar becomes an ordinary piece of copper, of smaller crystals, facing in all directions.

If the surface of the large crystal is nicked or dented, the structure in the neighborhood is changed in the same way. It is similarly affected by filing or polishing. When one of the bars is polished it is necessary to take off a mil or less at a time. Even then the structure of the new surface is altered. The condition is remedied by etching away the surface with the usual acid bath.

An etched bar of the copper appears to be rough. There seem to be alternate dark and light lines. The appearance of the lines is due to the fact that the acid etches more easily in some directions than in others. The directions in which it etches with the greatest difficulty are parallel to the axis of the crystal.

Externally the large, single copper crystals differ little from the usual metal. X-ray analysis, however, furnishes conclusive evidence that such a crystal has been produced. Doctor Davey, by means of special apparatus, was able to prove that he had one crystal. In the usual examination a small tube of finely powdered crystalline material is placed in the path of a narrow beam of X-rays of a specified wave-length. The substance turns the X-rays in different directions, according to the arrangement of the atoms in the minute crystals. A series of lines is produced on a photographic film, and these lines are used in calculations which reveal how the atoms are arranged and how far apart they are. Copper crystallizes in the face-centred cubic system, *i.e.*, the atoms are arranged at the corners of an imaginary cube, with another atom in the centre of each face. In studying the single crystal Doctor Davey revised the method of examination so that the large crystal was used, rather than crystalline powder. The specimen was swung slowly back and forth through an angle of thirty degrees, with the edge in the path of the X-rays. The rotation of the single crystal produced the same effect as using a stationary powdered sample, and a pattern was received on a stationary film. At the same time a moving film was used, mounted on the turntable with the crystal. If the specimen had not been a single crystal, no lines would have been obtained on the movable film, since the X-rays would have affected the entire film uniformly. The lines were obtained, however, and calculations based on a comparison of the two negatives showed that the axis of the crystal was parallel with the direction of cooling the ingot.

It is difficult to foretell the results which will follow a study of large metal crystals. It has been thought for several years that such specimens would have unexpected properties, and now the prophecy is substantiated.

Electrical Conductivity of Marble. G. STETTER. (*Phys. Zeit.*, Sept. 15, 1924.)—A piece of marble $83 \times 64 \times 22$ mm. had in fairly dry air a specific resistance of 2.6×10^{13} ohms. After the stone had

been a day in air saturated with water vapor, this quantity had sunk to 1 per cent. of its former value. In seven later days it went slowly down to 1.5×10^{10} ohms, at which value it remained fairly constant for eleven days longer. The surface resistance of the marble manifested about the same course of changes as its specific resistance.

Upon placing the block in dry air its specific resistance rose at first rapidly, then more slowly until 1.1×10^{15} was attained. After half an hour the conductivity of the surface ceased to be noticeable. Then for three hours the block was kept in boiling paraffin. The specific resistance of the impregnated block was found to be 10^{16} ohms. The author places the value for perfectly dry marble at 5×10^{15} ohms.

G. F. S.

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